PHYSICAL SCIENCE AND THE WOOLWICH EXAMINATIONS.

WE are glad to learn that several Members of Parliament are interesting themselves in this important matter, and that Sir John Lubbock and Sir Henry Roscoe have both put down notices of motion calling attention to the changes that it is proposed to make in the regulations for admission to Woolwich. We hope and believe that their efforts will result in a rectification of these ill-conceived regulations.

We have already shown in our previous articles on this subject how completely the new regulations fail to find any justification, so far as their treatment of experimental science is concerned. We have demonstrated, by an examination of the professional course of training which the successful cadets will go through when at the Royal Military Academy, that of the subjects of general education experimental science stands below mathematics alone in practical importance for Woolwich cadets ; whilst even a cursory inspection of the results of past examinations is sufficient to reveal the hollowness of the suggestion that in scientific subjects marks may be easily obtained by superficial study or cram. When we consider that the results of applying similar regulations in the case of the Sandhurst examinations are, or ought to be, familiar to the War Office authorities, it is astonishing that their extension to the scientific branches of the army should ever have been seriously contemplated.

The deliberate adoption of this scheme for selecting young men for a highly scientific profession, after the experience of several years had so completely established that it is eminently calculated to reduce the chances of candidates of scientific power to a minimum, can only be regarded as a remarkable example of official blundering. The rectification of the mistake is the more imperatively required because the treatment of natural science—that is, of candidates whose abilities are rather scientific than linguistic or mathematical—in public examinations has hitherto been altogether unsuited to the real wants of the age. Science in examinations being to a great extent a non-paying subject, the quality or even the existence of science teaching is regarded, at the best, as a matter of secondary importance in many or most of our schools. The question, therefore, deserves the closest attention from all who hold that it is absolutely essential that there shall be a steady and sure advance in the standard of elementary science teaching in this country.

In his reply to Mr. Howorth, the Secretary of State for War is stated to have said that these Woolwich regulations had been considered by a " strong Committee." It would be interesting to know of whom this Committee consisted, and whether it was strong from a military or an educational point of view. Such information as we have been able to obtain leads us to conclude that it was a military Committee, and that though, as such, it was no doubt eminently fitted to come to wise conclusions on military questions—such, for example, as the proper training to be given to successful cadets after their admission to the Royal Military Academy—it was

by no means composed of men equally fitted by experience to deal with the other side of the question. It is surprising to find that this important change, which will profoundly affect much of the higher school work of the country, was apparently decided upon without, or almost without, consultation with those most experienced in such questions This helps us to understand how it has happened that regulations not altogether unsatisfactory, and to which many places of education had adapted themselves, often at considerable expense and trouble, are suddenly to be displaced by others that are open to the gravest objections.

The new regulations seem to have almost every order of fault. They will be unfair to the candidates, leading to the rejection of those best fitted for the work to be done. It is to be feared, too, that they will encourage residence and study abroad, with the consequent loss of the valuable moral and physical training that can be had only in England. They will also act prejudicially on the general tendency of school education. We hope we may soon hear that better counsels have prevailed, and that these unfortunate regulations are to be replaced by others more in accordance with modern needs and ideas.

TEA CULTIVATION IN INDIA.

Die Theekultur in Britisch-Ost-Indien, im fünfzigsten Jahre ihres Bestandes, Historisch, Naturwissenschaftlich, und Statistisch. Dr. Ottokar Feistmantel. (Prague : O. Beyer, 1888.)

THE subject of tea cultivation in India is one to which innumerable writers have devoted their attention, and not the least valuable portion of Dr. Feistmantel's work, " Die Theekultur in Britisch-Ost-Indien," is the bibliography of the subject with which, while recording his indebtedness for much of his information to many of the English and German authors enumerated, he commences his remarks. In his preface he explains that in the course of an address on the products and exports of British India, recently delivered by him in Prague, he alluded to the fact that on the Continent of Europe tea was generally known only as either Russian or Chinese, and that it was barely known that India produced a large and annually increasing quantity of high-class teas, which are largely used in London for mixing with and improving China tea. The correspondence which ensued when these remarks were reported by the local press induced him to publish the present work as the result of information he had the opportunity of collecting while serving in India for eight years as palæontologist to the Geological Survey.

It is Dr. Feistmantel's aim to place before the German-speaking peoples of the Continent as complete an exposition of the conditions of the tea industry in India as has already been laid before English-speaking people by other writers ; and he therefore begins with an abstract of the early history of the tea-plant in India, the dates of its first discovery as an indigenous shrub, and its first introduction into the different districts in which it is now cultivated. He mentions the first export from India to England in 1838 of twelve chests of tea, which sold for 19*s.* 5*d.* per pound.

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He points out the differences between the indigenous, the "China," and the hybrid varieties of the plant which are cultivated in India, and enumerates the various pseudo-teas which are known either in the frontier countries of India or in other countries : such as *Osyris nepalensis* or *arborea*, in Kumaon, Garhwal, and lately in Kashmir ; *Elæodendron persicum*, in Burmah, from which, when mixed with oil, salt, garlic, and assafœtida, is prepared the nauseous compound, to European taste, known as "pickled tea" ; *Ilex paraguayensis*, the Paraguay tea, or "Mate," of South America ; *Ledum palustre*, or Labrador tea ; the Tasmanian tea, made from various varieties of *Melaleuca* and *Leptospermum ;* and the Faham tea, *Angræcum fragrans* of Mauritius ; and others.

The number of plantations in the various provinces, area under cultivation, and annual yield of tea for all India, are given in detail; and the differences between the various kinds of China and Indian tea, as proved by analysis, are very fully treated of. The principal black teas made in India are flowery pekoe, orange pekoe, souchong, pekoe souchong, congou, and bohea ; as also the several varieties of broken leaf, such as broken pekoe, pekoe dust, &c. All these are not, as is commonly supposed, the produce of different plants, but are prepared from one and the same plant, the classification being caused by the difference of age and development of the leaves used for the several varieties. The principal kinds of green tea are gunpowder, hyson, and young hyson, and these are manufactured almost exclusively in the North-West Provinces and Kangra.

It may be accepted as a fact that Indian tea is very rarely adulterated, being packed on the plantation, and shipped direct from the planter to the market ; but "China tea" passes through many hands before it is packed for shipment, and is frequently mixed with willow or other leaves, or with artificial colouring-matter. But the adulterated tea is not now readily saleable in London, and is therefore re-exported to the Continent. A direct importation of tea from India to the Continent would insure the purity of the supply.

In a lecture given before the Society of Arts, in May last, by Mr. J. Berry White, and quoted by Dr. Feistmantel, a table is given showing the steady rise of the Indian tea crop from 232,000 pounds in 1852 to 76,585,000 pounds in 1886 ; and Mr. White estimated that the crop for 1887 would not fall far short of 90,000,000 pounds. The amount of tea exported from India between October 1, 1885, and September 30, 1886, is officially returned as 68,784,249 pounds, of which 66,640,749 pounds went to England. Nearly the whole of this tea is consumed in Great Britain, a small quantity being sent to the Continent mixed with inferior China teas, and consequently sold as China tea. The percentage of Indian tea used in England has also been steadily rising, for whereas in 1865 China tea formed 97 per cent. of the entire consumption, in the first quarter of 1887 the proportion was 51 per cent. of Indian to 49 per cent. of China tea.

Notwithstanding the steadily increasing production in India, China tea is still imported into the country ; in 1885-86 about four million pounds were imported, but mainly into Bombay, where none is grown, and much of it for re-export to the Persian Gulf, Afghanistan, and some to Trieste, where it arrives as Indian tea.

Statistics concerning the consumption of tea show that the greatest tea-drinkers are the Australians, who in 1881 consumed 81 ounces per head of the population. England ranked next with 73 ounces, while the United States of America came next with 21 ounces. Russia, Belgium, Holland, and Denmark rank highest among Continental nations as tea-drinkers, but they only consume from 7 to 8 ounces per head of the population.

Dr. Feistmantel fully indorses the prevalent English opinion as to the superiority of Indian to China tea, and attributes its being almost unknown on the Continent mainly to the fact that "China tea" is a much older, and therefore better known, product throughout Europe. Even in England Indian tea took years to establish its reputation. It will in the end be as much appreciated on the Continent as it is in this country if a few merchants and tradesmen in different Continental cities, whose commercial standing will be a guarantee for the purity of the goods they supply, are induced to keep it.

A special chapter is devoted to the cultivation of tea in Ceylon, and shows the marvellous progress made by this new industry in consequence of the coffee disease having caused the conversion of so many coffee plantations into tea plantations. In 1875 only 1080 acres were under tea, whereas in 1885 no less than 102,000 acres were occupied by it, and the exports rose from 282 pounds in 1875-76 to nearly four million pounds in 1884-85. The plantations are principally in the western and southern provinces of Ceylon.

Dr. Feistmantel's work concludes with an interesting chapter on caravan teas, compiled from an article by Herr Walter Japha, published in the *Revue Coloniale Internationale* for September–October 1887.

Some amongst us are apt to feel a certain amount of jealousy at the not infrequent employment of foreigners in Government appointments, and this feeling is perhaps intensified by the knowledge that in this matter, as in Free Trade, there is no apparent reciprocity—for we seldom hear of the employment of Englishmen by Continental Governments ; but the present is an instance, and by no means a solitary one, of the great service done to us by foreigners who avail themselves of the information they have collected in the course of their employment by our Government to diffuse among their fellow-countrymen such an intelligent knowledge of the productions of our distant possessions as is calculated to largely benefit our commerce by leading to an extensive demand for the goods of which they write.

It would seem, however, scarcely just that the work of diffusing this knowledge should be left to other nations, seeing that the benefits are to be reaped by ourselves. It is hardly likely that in England it will be recognized, as it is in some other countries, to be part of the duties of any Government Department ; but why should it not be part of the work of such a body as the London Chamber of Commerce, or the new Imperial Institute, to disseminate information regarding our Colonial and Indian products among Continental nations, and to translate and circulate any useful works on commercial and kindred subjects, published in foreign languages, among such classes of the community as they would be likely to interest ?

J. R. ROYLE.

LIVING LIGHTS.

Living Lights: a Popular Account of Phosphorescent Animals and Vegetables. By C. F. Holder. (London: Sampson Low, Marston, Searle, and Rivington, 1887.)

THIS pleasant volume of 167 pages is intended for young students of science, " their unscientific elders, and the boys and girls in general who have not yet had their interest aroused in Nature's works." The field covered is very wide, and the book is truly Germanic in its meanderings. The author would appear to be under the spell of those who "not only know all that is known by other people, but also all that they themselves imagine, which nobody else can possibly know." When it is said that the results obtained by the expeditions of the *Challenger, Talisman, Albatross, Travailleur,* and *Magenta,* are incorporated, no one can raise the charge of antiquity. The author discusses all possible sides of his subject, from luminous man to cosmic dust in its relation to sun-glow and even luminous paint itself, which was, as is well known, anticipated by the Chinese (oh, Mr. Balmain !). It must not, however, be imagined that the volume is a mere compilation. Quite the reverse ; for, while the author embodies much that is original, he incorporates manuscript notes, placed at his disposal by our veteran Gosse, and by luminologists such as Giglioli, Dubois, and others.

Technicalities are for the most part relegated to an appendix, with full references to authorities ; the result being that while the book, as a whole, furnishes the specialist with a work of reference the body of it is rendered assimilable by the feeblest tyro. The subject is introduced by a consideration of the bottom of the ocean, which the author naïvely terms the "lower firmament"—an idea which he elaborates in the subsequent chapters, treating of "meteors" and "fixed luminaries" of the sea. We meet with many friends of our youth, such as, for example, M. de Tessan's well-worn picture of the phosphorescent sea at Simon's Town, with its accompanying description.

By way of relieving monotony, anecdotes and similes are freely intercalated with the text. Some of the latter are very happy, as, for example, the comparison drawn between the blind-man and the *Bathypterus* (p. 92). On p. 13 we read : " By having a companion to keep up a continuous motion of the (luminous) water, I have almost been able to read the print of a newspaper by the light of these disintegrated (animal) forms "—a literal stern reality this, sufficient to break the heart of a Ruskin.

The author appears to be suffering under a phosphorescence mania. He leads off with the rather extravagant statement, "Among the revelations of modern science none have a more absorbing interest than those relating to the illumination of the deep sea." He is, moreover, a genuine enthusiast, and, like all such, sees the salvation of his race in his own hobby, for he gives it as his opinion (p. 41) that "the discovery of the secret of phosphorescence, and its practical application to the wants of mankind, would result in revolutionizing present systems ; a heatless, inexpensive, inextinguishable light being the perfection of possibilities in this direction." Similar sentiments are expressed in the peroration : these we commend to the physicist.

The book is exceedingly well got up, and illustrated by twenty-six plates, most of which have been especially designed for it. One of these, representing the now famous giant Pyrosoma of the *Challenger,* in size proportionate to that of a man, is especially striking, and the publishers have, very properly, reproduced it on the cover. We would, however, suggest that, in the case of sponges and corals more especially, the animals themselves, and not their mere skeletons, should be delineated ; the course here adopted is too suggestive of a " matching " of ordinary museum specimens for the sake of effect. Here and there we note a looseness of style and expression such as is frequently met with in a first issue. The book—strictly a general treatise on luminosity—is a conscientious exposition of a fascinating subject, sound though superficial, and in no sense sensational. We wish it success.

OUR BOOK SHELF.

Food Adulteration and its Detection. By J. P. Battershall, Ph.D., F.C.S. (New York : Spon, 1887.)

THE most striking points of this book are the photographic reproductions of various food-stuffs : starch-grains, fat-crystals, also margarine, milk, tea-leaves, &c. In the introduction Dr. Battershall laments the general inefficient state of the law in America, which would apply very much more forcibly to us, regarding adulteration.

The author does very good service in his introduction, drawing attention to the statistics of recent adulteration. From one table, taken from the work of the Public Analysts' Society in England, it appears the percentage of adulteration has not decreased in any appreciable degree, having been 18·10 per cent. in. 1875-76, and 17·47 in 1880, and 16·4 in 1883. The Annual Report of the New York City Board of Health for 1885 furnishes some statistics of adulteration which are by no means pleasant, and show a not very high commercial morality, although the majority are said not to be injurious adulterations—merely fraudulent. The author is quite right when he says "that attempts to awaken public interest in the subject are only of real service as they are conducive to the adoption of more advanced and improved measures for the suppression of the practice."

Generally, the subjects are treated in the book in a very practical manner, and a good deal of information is also contained under each heading. Regarding the adulteration of wines, for instance, a good many interesting receipts for making wines are given, and similarly in the case of spirits and liquors. The section on water is a good *résumé* of processes of water analysis. Prominence is rightly given to Prof. Mallet's very sensible conclusions as to the value of analytical methods in respect to the hygienic character. Dr. Koch's biological method, cultivation in prepared gelatine, is mentioned, and a plate showing the living forms in Croton water and Brooklyn water is given, but we are not frightened by any alarmist theories or statements as to the injurious nature of these organisms ; indeed, we are told that the greater number are unobjectionable, and frequently even of service, which is doubtless the case. The really active Bacteria are much less impressive in appearance.

There is a pretty long chapter on legislation in the United States on adulteration, which is not of much use, but is still interesting, to an English reader. The bibliography is very useful. Altogether it is a readable and useful book, and will doubtless meet with a good reception.　　　　　　　　　　　　　W. R. H.

Dynamics and Hydrostatics. By R. H. Pinkerton, B.A. (London : Blackie and Son, 1888.)

THIS is a first course of dynamics intended for the use of science classes and colleges, and specially adapted to the requirements of the Science and Art Examinations in theoretical mechanics. The subject is treated mathematically, but the mathematical knowledge required for an intelligent perusal of the book is limited to elementary algebra and trigonometry. The fundamental units are thoroughly well explained, and, which is saying a great deal, they are used consistently throughout. Every important proposition is followed by a number of good examples fully worked out, and many others are given as exercises.

The book is excellently adapted to the Second Stage of the Science and Art Syllabus, and teachers will not have much difficulty in selecting the portions suitable for students working for the First Stage. It is also well adapted for the use of students working at the subject for the London Matriculation and other University Examinations. But, notwithstanding these qualifications, it is thoroughly conscientious. In fact, from a mathematical point of view, the book leaves nothing to be desired, but in this practical generation a greater number of illustrations from every-day life would not have been out of place.　　　　　　　　　　　　　　　　A. F.

Geography for Schools. By Alfred Hughes, M.A. Part I. Practical Geography. (Oxford: At the Clarendon Press, 1887.)

THERE are many signs that the study of geography will in future take a much more important place in the ordinary school course than has hitherto been assigned to it. Even from the point of view of those severely practical persons who care little about the purely intellectual aspects of education, there can be no doubt as to the value of the kind of geographical knowledge with which this book is chiefly concerned ; and the subject, if properly treated, is one in which young scholars may easily be led to take genuine interest. The present volume will be of great service to schoolmasters who may wish to make a fresh start in geographical teaching. It is based, as Mr. Hughes explains, on the results of seven years' experience in the modern side at the Manchester Grammar School ; and no one who examines the book will be surprised that he has found it possible, within the limits of an ordinary term's geographical course, to give instruction on many classes of problems which are not usually treated at school. He begins with the consideration of latitude and longitude, and with rules for the drawing of maps from the atlas and from memory. He then deals with the measurement of the distance between two places on the earth's surface, and explains the rotation of the earth, with the consequent difference in the time of day at two places on the earth. The remaining subjects are the apparent movements of the fixed stars ; the Pole star ; Polar distance ; the apparent movements of the sun ; the seasons ; meridian altitude of the sun ; declination ; the length of day and night at any time and place ; the sun's altitude ; place of sunrise and sunset ; the length of twilight ; apparent and Greenwich mean time ; movements of the earth ; the length of shadows ; the distance to be seen from mountain summits ; the trade winds ; and the calendar. The questions connected with these subjects are discussed in a way that secures the combination of geography, geometrical drawing, arithmetic, and the elementary ideas of geometry ; and the author's aim is to induce the student to think for himself, rather than to burden his memory with disconnected facts. It is hardly necessary to say how much better this is than the learning of the names of capes, mountains, rivers, &c., by heart. With such a work in their hands, teachers should be able to make lessons in geography a most useful introduction to the study of some important branches of scientific method.

Key to Todhunter's Differential Calculus. By H. St. J. Hunter, M.A. (London: Macmillan and Co., 1888.)

THIS "Key" will be extremely useful to those who are teaching the subject, but more so to those who are getting it up by themselves. The examples are worked out in a clear and intelligible manner, the geometrical problems being so worded that the student can supply figures to enable him more readily to follow the reasoning. To the chapters on " Curve Tracing " and " Miscellaneous Propositions " the author has added figures ; and in the solutions to some of the examples in chaps. xi., xiii., xv., xx., and xxii., improved methods have been adopted, making the book more useful and complete. Great care seems to have been taken to insure accuracy.

Electrical Instrument Making for Amateurs. By S. R. Bottone. (London : Whittaker and Co., 1888.)

IN this little book the author has placed before the reader very good and economical methods of making the more useful pieces of electrical apparatus, using only tools of the simplest kind, such as may be found in any household. The instructions are given in a clear and simple manner, and are illustrated by woodcuts, showing the various parts of the apparatus, with the proportions marked on them. Those who are attending courses of lectures on this subject will find this volume immensely useful, as a more thorough and practical insight is obtained by making and using these instruments, however rough, than by mere reading.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Language = Reason.

PROF. ST. GEORGE MIVART has read my letter on " Language = Reason " in NATURE of February 2 (p. 323) with very great care, and I feel grateful to him for several suggestive remarks. But has he read the heavy volume to which that letter refers—my " Science of Thought " ? I doubt it, and have of course no right to expect it, for I know but too well myself how difficult it is for a man who writes books to read any but the most necessary books. I only mention it as an excuse for what might otherwise seem conceited—namely, my answering most of his questions and criticisms by references to my own book.

Prof. Mivart begins by asking why I should have explained reasoning by reckoning.

Now, first of all, from an historical point of view—and this to a man who considers evolution far more firmly established in language than in any other realm of Nature is always the most important—the Latin *ratio*, from which came *raison* and our own *reason*, meant originally reckoning, casting up, calculation, computation, long before it came to mean the so-called faculty of the mind which forms the basis of computation and calculation, judgment, understanding and reason.

Secondly, I began my book on the " Science of Thought " with a quotation from Hobbes, that all our thinking consisted in addition and subtraction, and I claimed the liberty to use the word thinking throughout my own book in the sense of combining. Such a definition of thinking may be right or wrong, but provided a word is always used in the sense in which from the beginning it has been defined there can at all events be no misapprehension nor just cause of complaint on the part of the critic.

What I meant by combination, or by addition and subtraction being the true character of thinking, I explained very fully. "Any book on logic," I said, "will teach that all our propositions are either *affirmative* or *negative*, and that in acquiring or communicating knowledge we can do no more than to say that A is B, or A is not B. Now, in saying A is B, we simply add A to the sum already comprehended under B, and in saying A is not B, we subtract A from the sum that can be comprehended under B. And why should it be considered as lowering our high status, if what we call thinking turns out to be no more than adding or subtracting? Mathematics in the end consist of nothing but addition and subtraction, and think of the wonderful achievements of a Newton or a Gauss—achievements before which ordinary mortals like myself stand simply aghast."

Prof. Mivart holds that there are but two forms of intellectual activity: (1) acts of intuition, by which we directly apprehend certain truths, such as, *e.g.*, our own activity, or that A is A; and (2) acts of inference, by which we indirectly apprehend others, with the aid of the idea "therefore."

There is a wide difference between our apprehending our own activity and our apprehending that A is A. Apprehending our own activity is inevitable, apprehending that A is A is voluntary. Besides, the "therefore" on which Prof. Mivart insists as a distinguishing feature between the two forms of thought is present in the simplest acts of cognition. In order to think and to say "This is an orange," I must implicitly think and say, "This is round, and yellow, has a peculiar skin, a sweet juice," &c.; *therefore* it is an orange. The "therefore" represents in fact the justification of our act of addition. We have by slow and repeated addition formed the concept-name orange, and by saying "This is an orange," we say no more than that we feel justified, till the contrary is proved, in adding this object before us to the sum of oranges already known to us. If the contrary is proved, we subtract, and we add our present object either to the class and name of lemons, citrons, &c., or to a more general class, such as apples, fruit, round objects, &c. We ought really to distinguish, as I have tried to show, not only two, but four phases in every act of cognition, viz. sensation, perception, conception, and naming; and I contend that these four phases, though distinguishable, are not separable, and that no act of cognition is perfect without the last phase of naming.

But how is it, Prof. Mivart continues, that different words in our language have one meaning, and different meanings one word? Does not this show that thought and language cannot be identical?

It has been the principal object of all my mythological studies to account not only for the origin of *polyonymy* and *homonymy*, but to discover in them the cause of much that has to be called mythology, whether in ancient tradition, religion, philosophy, or even in modern science. I must therefore refer Prof. Mivart to my earlier writings, and can only mention here a few well-known cases of mythology arising from polyonymy and homonymy.

We can easily understand why people should have called the planet Venus both the morning and the evening star; but we know that in consequence of these two names many people have believed in two stars instead of one. The same mountain in Switzerland is called by the people on the south side *Blackhorn*, by the people on the north side *Whitehorn*, and many a traveller has been misled when asking his way to the one or the other. Because in German there are two words *Verstand* and *Vernunft*, originally meaning exactly the same thing, German metaphysicians have changed them into two distinct faculties, and English philosophers have tried to introduce the same distinction between the understanding as the lower and reason as the higher faculty.

Nothing is really easier to understand, if only we consult the ancient annals of language, than why the same object should have had several names, and why several objects should have had the same name. But this proves by no means that therefore the name is one thing and the concept another. We can distinguish name and concept as we distinguish between the concave and convex sides of a lens, but we cannot separate them, and in that sense we may call them inseparable, and, in one sense, identical.

Lastly, Prof. Mivart starts the same objection to my system of psychological analysis which was raised some time ago in these columns with so much learning and eloquence by Mr. Francis Galton. He appeals to his own experience, and maintains that certain intellectual processes take place without language. This is generally supposed to put an end to any further argument, and we are even told that it is a mistake to imagine that all men are alike, so far as their psychological processes are concerned, and that psychologists should study the peculiarities of individuals rather than the general character of the human intellect. Now, it seems to me that *l'un n'empêche pas l'autre*, but that in the end the object of all scientific inquiry is the general, and not the individual. The true life of language is in the dialects, yet the grammarian aims at a general grammar. In the same way the psychologist may pay any amount of attention to mere individual peculiarities and idiosyncrasies; only he ought never to forget that in the end man is man.

But it does not even seem to me that intellectual processes without language, as described by Mr. Galton and Prof. Mivart, are at all peculiar and exceptional. I have described similar cases, and tried to account for them, in different parts of my book. If Prof. Mivart says that "a slight movement of a finger may give expression to a meaning which could only be thought in words by a much slower process," I went much further by saying that "silence might be more eloquent than words."

Mr. Galton asked me to read a book by Alfred Binet, *La Psychologie du Raisonnement*, as showing by experiments how many intellectual acts could take place without language. I read the book with deep interest, but great was my surprise when I found that M. Binet's observations confirmed in the very strongest way my own position. I had shown how percepts—that is, images—could exist with a mere shadow of language, and that nothing was more wonderful than what Leibniz called the algebra of thought. Now, what do M. Binet's experiments prove? That there are two kinds of images, the *consecutive*, reproduced spontaneously and suddenly, and the *memorial*, connected with an association of ideas. The *consecutive* image, a kind of impression *avant la lettre*, may reappear long after the existing sensation has ceased to act, and it reappears without any rhyme or reason. But how are the memorial images recalled, seen by people, such as M. Binet describes, in a state of hypnotism? Entirely by the word. Show a hypnotized patient her portrait, and she may or may not recognize it. But tell her, in so many words, "This is your portrait," and she will see her likeness in a landscape of the Pyrenees (pp. 56–57). M. Binet is fully aware of what is implied by this. Thus, on p. 58, he writes: "*L'hallucination hypnotique est formée d'un image suggérée par* la parole." So, again, when describing the simplest acts of perception, M. Binet explains how much is added by ourselves to the mere impressions received through the senses by "*ce qu'on croit voir*," by "*ce qu'on croit sentir*," and by "*le nom qu'on croit entendre prononcer*." The facts and experiments, therefore, contained in M. Binet's charming volume seem to me entirely on my side, nor do I see that that thoughtful observer has ever denied the necessity of language or signs of some sort for the purpose of reasoning, nay even of imagination.

I find it difficult to answer all the questions which the Professor has asked, because it would seem like writing my own book over again. However, I shall confess that I have laid myself open to some just criticism in not renouncing altogether the metaphorical poetry of language. I ought not to have spoken of Truth as a kind of personal being, nor of Reason as a power that governs the universe. But no astronomer is blamed when he uses the old terminology of sunrise and sunset; no biologist is misunderstood when he speaks of mankind; and no philosopher is denounced when he continues to use the big I instead of "succession of states of consciousness." If, therefore, I said that I recognized in evolution the triumph of reason, I meant no more than that I could not recognize in it the triumph of mere chance. Prof. Mivart imagines that I misunderstood what the biologist means by the survival of the fittest. Far from it, I understand that phrase, and decidedly reject it. For, either the survival of the fittest means no more than that that survives which is able to survive,—this would be mere truism and a patent tautology,—or, if we take in the whole circumstance of Nature, the survival of the fittest implies some kind of inherent fitness and reasonableness. Prof. Mivart writes: "What there is less reasonable and right in a Rhytina than in a Dugong, or in a Dinornis than in an Apteryx, would, I think, puzzle most of our zoologists to determine; nor is it easy to see a triumph of reason in the extermination of the unique flora of St. Helena by the introduction of goats and rabbits." No doubt, it is not easy to see this. But need I remind Prof. Mivart that many things may be true, though it is not easy to see them? We often do what we think is reasonable and right, though we seem to see nothing but mischief to ourselves and others arising from our acts. Why do we do this? Because we believe in the ultimate triumph of reason

and right, though it may take millions of years to prove that right is right. I have the same faith in Nature; and, taking my stand on this scientific faith, I believe that natural selection must in the end prove rational selection, and that what has vaguely been called the survival of the fittest will have to be interpreted in the end as the triumph of reason, not as the mere play of chance.   F. MAX MÜLLER.

Oxford, February 21.

"Coral Formations."

CAPTAIN WHARTON'S paper on coral formations in last week's NATURE (p. 393) will have been read with great interest by all who have examined and studied coral reefs. It is unlikely that any objections will be raised to the illustrations he has brought forward of how the coral plantations may be built up from deeply submerged banks, and eventually formed into complete atolls and barrier reefs at a great distance from continental and other shores. The mode of formation has been dwelt upon by Le Conte and Guppy in the case of barrier reefs, and I have pointed out the same thing in my remarks about the Maldive and similar atoll groups. The instances cited by Captain Wharton are of great value, especially as he has been able to consult large manuscript plans.

Captain Wharton apparently considers that the solution of carbonate of lime by sea-water plays no important part in deepening, widening, and modifying the form of such atolls and barrier reefs; in this I cannot agree with him.

By reference to what is now taking place in Nature, as well as to experiments conducted in the laboratory, it has been shown that the solution of the carbonate of lime of dead shells and skeletons by the sea is as constant and universal as its secretion by the living organisms. From some considerations which I recently laid before the Royal Society of Edinburgh, it is probable that there is more secretion and deposition of carbonate of lime in the ocean, as a whole, than removal by solution, and it is almost certain that at the present time there is a vast accumulation of carbonate of lime going on within the coral-reef regions of the ocean. The amount of secretion becomes less with increasing depth beyond one hundred fathoms, and laboratory experiments under great pressures have shown that the rate of solution becomes greater with increasing depth; but both processes are always in action wherever there are life and growth, death and decay. In some regions secretion is in excess, and there is a formation of calcareous deposits; in others solution is equal to secretion, as over the red clay areas of the ocean. Again, solution may be in excess of secretion, as in the larger and more perfect coral lagoons. The *rôle* of carbonate of lime in the ocean may not inaptly be compared to that of aqueous vapour in the atmosphere over land surfaces. Where precipitation is in excess of evaporation, fresh-water lakes are formed, and rivers carry the surplus water down to the ocean; where evaporation is in excess, there is a formation of inland drainage areas, deserts, and salt lakes.

In small coral atolls the periphery is large relatively to the size of the lagoon, and the secretion of lime and formation of coral sand are greatly in excess of the solution that takes place, hence the lagoon becomes filled up; in it are frequently found deposits of sulphate of lime, guano, magnesian and phosphatic rocks. On the other hand, when a comparatively large atoll reaches the surface, the periphery being small relatively to the size of the lagoon, there is less secretion and formation of coral sand by the living outer surface than is removed in solution from the lagoon; it is in consequence widened, deepened, and reduced to a more or less uniform appearance, while the islands on such reefs never, so far as I know, contain deposits of sulphate of lime, guano, magnesian or phosphatic rocks. On open banks, such as the Macclesfield and Tizard Banks, the coral sand is generally largely made up of bottom-living Foraminifera, Polyzoa, Serpulæ, and Calcareous Algæ, and the bank may be rising from the secretions of these organisms; but when the peripheral reefs reach the surface the conditions become more or less inimical to vigorous growth, and in a perfect atoll the fine calcareous mud is removed at a relatively rapid rate.

My answer to Captain Wharton's question is that in all normal conditions the extent of surface in the shell, coral, or fragment of coral sand exposed to the action of sea-water compared with the mass determines the rate at which these organisms will disappear in solution. It is improbable that this action is extremely slow at the bottom of the deep lagoons. Independently of the mixing by convection currents, even a very slight wind over the surface of the lagoon will set the whole water in motion. This is clearly shown by my observations in the western lochs of Scotland, which are much deeper than any lagoon; a moderate breeze produces motion at a depth of sixty fathoms in a very short space of time. The water mixed up with the mud at the bottom is thus changed long before the point of saturation is reached.

I have never seen any wide extent of fringing reef but what was very deeply cut up with channels, and from Captain Wharton's own description this appears to be the case at Rodriguez. That a ship channel has not there been formed is probably due to the shallow water surrounding the island and the probably rapid growth outward of the reef; the average depth outside the reef is usually less than ten fathoms, and at a distance of two miles seaward it is only from twenty to thirty fathoms. In some instances the large proportion of Calcareous Algæ on the reefs appears to compensate for the removal in solution, and thus to retard the formation of ship channels.

I doubt if any recent writer has attempted to give an "explanation which will fully account for the almost infinite variety of coral formations." It is unnecessary to state that each reef must have peculiarities depending on the nature and form of its foundation, and the meteorological and other conditions of the seas in which the reef is situated; it is only by a careful and detailed study of all these conditions that the peculiarities of any individual reef can be fully explained. At the same time it appears to me beyond doubt that the general and well-known characteristic features and form of coral reefs can be accounted for by reference to certain general considerations, chief among these being the vigorous growth of reef-forming species in positions and at depths where the supply of pelagic oceanic organisms, which form their food, are most abundant, and the removal of dead coral and coral debris wherever this is exposed to the action of sea-water.

Captain Wharton calls attention to our imperfect knowledge of the coral groups of the Pacific, but he understates the case in saying "that the waters of the Fiji and the Society Islands are the only ones which can be said to be in any sense surveyed." Cook, Kotzebue, Duperrey, Beechy, and Wilkes have given running surveys of many of the Paumotus, and we know something about the depths inside and outside of a good many of them. We know much about the islands containing guano. The French have made some excellent charts of the New Caledonia reefs, and the Americans have done the same for some of the Hawaiian Islands. Captain Wharton will acknowledge that we have a splendid survey of the Maldives, the most extensive group of atolls in the world; this British Survey number 602. Other groups in the Indian Ocean are well surveyed, and nearly all the Atlantic reefs have been correctly laid down on charts.

I feel sure that all who take an interest in this subject will hope for many more contributions from Captain Wharton's pen on coral formations.   JOHN MURRAY.

I HAVE read with great interest the article on coral formations in your last number (p. 393), by Capt. Wharton. It is not because I wish to claim to have anticipated the views which he gives as to the formation of atoll lagoons and barrier reef lagoons that I am writing to state that at the very date of the publication of Capt. Wharton's article I was engaged in writing a paper on coral formations, based upon a study of living corals at Diego Garcia, and on a consideration of the great submerged atolls known as the Great Chagos Bank and the Pitt and Centurion Banks, situated north and west of that island, in which I arrive at conclusions nearly identical with his. It has seemed to me, as it has to him, that the solution of dead coral rock in the interior of a reef does not sufficiently account for the formation of lagoons, and that the true cause of the atoll and barrier lagoons surrounded either by a reef which is awash, or by a strip of low land, lies in the peculiarly favourable conditions for coral growth present on the steep external slopes of the reef. In Diego Garcia I observed that although the shore reefs are for the most part covered with 1 or 2 feet of water, even at the lowest spring tides, yet their flat surfaces are nearly invariably barren of growing coral. Just at their edges, however, and on the steep external slopes beyond the edges, reef-building corals grow luxuriantly. According to Capt. Moresby, quoted by Mr. Darwin in his book on "Coral Reefs," the flat surface of the rim of the Great Chagos Bank is barren of living corals,

just as are the shore reefs of the neighbouring atoll of Diego Garcia ; but the lagoon contains many knolls abundantly covered with living coral, and there is reason to think that living coral also occurs on the external slopes at Diego Garcia. Unlike Capt. Wharton, I do not consider the favourable conditions for coral growth on the external slopes to be connected with a better food supply, for this would be at variance with the existence of thriving coral patches within the lagoon, which, as I have seen at Diego Garcia, bear no relation to the lagoon mouths, through which food-bearing currents might be supposed to enter to the interior. Indeed, at the last-named atoll some of the most luxuriant coral patches are found at the south end of the lagoon, furthest away from the lagoon outlet. The favourable conditions are due, I believe, to the action of currents on coral growth. I noticed at Diego Garcia, and Dr. Hickson has made similar observations in the reefs near North Celebes, that corals do not thrive where they are subjected to the direct action of a strong current, nor do they grow in still water, where they are killed by the sand deposited upon them, but they flourish in places where a moderate current flows over them, not so strong as to dash them to pieces, but strong enough to prevent deposition of sand. Such conditions are found everywhere on the external slopes. At the side where a current impinges directly on a slope, the deeper parts of the current strike the slope first, and are in part thrown upwards over the sloping surface, thus moderating the direct force of the more superficial part of the same current. The main part of the current flows tangentially around the obstruction, and thus affords favourable conditions at the sides of the atoll or reef, and finally, on the side furthest from the current, the back-wash causes weak superficial currents which are also highly favourable to coral growth. Thus the coral grows to the greatest advantage around the periphery of a reef, and, as Capt. Wharton says, a ring-shaped reef is the result, and no theory of solution is required to explain the central depression.

Capt. Wharton states that live coral exists in abundance on the rim of the Tizard Reef. It is not clear whether this means on the external slopes and on the extreme edge of the reef, or on the flat upper surfaces of the reef itself. From what I have observed at Diego Garcia, it appears to me hardly probable that the latter can be the case. Coral debris, torn from the corals growing on the slopes, is always carried across those flat surfaces in such quantity as to destroy any living corals upon them. In some cases corals may grow there, but then there are other favourable conditions neutralizing the effect of the debris. I am hoping soon to publish a full account of my observations at Diego Garcia.　　　　G. C. BOURNE.

Anatomical Department, Oxford, February 28.

Natural Science and the Woolwich Examinations.

IN accordance with Mr. Irving's recommendation, I have carefully considered the letter in the *Times* from the head master of Clifton College ; but, with all due respect to his distinguished position, I find myself unable to accept his conclusions. Men of science will pardon me, if I ask them to examine facts, rather than to follow blindly even the highest authority.

The obligatory mathematics to be required from candidates for Woolwich are defined as follows in the official regulations, dated December 1887 :—

"Algebra up to and including the binominal theorem ; the theory and use of logarithms ; Euclid, Books i. to iv. and vi. ; plane trigonometry up to and including the solution triangles ; mensuration ; statics—the equilibrium of forces acting in one plane and of parallel forces, the centre of gravity, the mechanical powers ; dynamics—uniform, uniformly accelerated, and uniform circular motion, falling bodies and projectiles *in vacuo*. (Analytical methods of solution will not be required.)

"N.B.—A thorough knowledge of each of the above branches of mathematics will be required."

This amount of mathematics is not beyond the reach of a fairly intelligent lad of seventeen who has been properly taught.

The inductive process which leads Mr. Irving to denounce so severely my supposed inappreciation of the value of experimental demonstration, laboratory training, and field work is hardly worthy of so eminent a teacher. Although there are good grounds for my opinion that chemistry, physics, and geology, are not good educational subjects for ordinary lads under sixteen, I am entirely consistent in the expression of my regret that the

War Office should have thought it desirable to discourage these sciences. Your able article conclusively proves that these subjects cannot be hastily and superficially learned in such a way as to gain unmerited marks. There are youths with apt intelligences, quick eyes, and skilful fingers, who ought to be allowed the advantage of their scientific capacity in the Woolwich competition. But I am unable to see that Mr. Irving's suggestion would do justice to these. A candidate who offered optional mathematics, one language, and two sciences, would be placed at a great disadvantage with those offering optional mathematics, and three languages, both on account of the lower maximum, and also because, with the same relative proficiency, it is so much harder to score in mathematics and experimental sciences and geology than in languages. I therefore respectfully submit that all who have the interests of science at heart should urge that the maximum should be raised to 3000 marks, but I do not think it would be desirable to allow candidates to take more than one subject from Class II., as it would tend to the neglect of more important studies.

2 Powis Square.　　　　HENRY PALIN GURNEY.

International Tables.

I AM instructed by the Meteorological Council to request your insertion of the following notice :—

The International Meteorological Congress, which met at Rome in 1879, recommended that a series of international tables should be prepared and issued.

The work was ultimately intrusted to a Sub-Committee, consisting of Prof. Wild and Prof. Mascart.

The Sub-Committee has prepared a scheme of tables, which has met with a general acceptance among the heads of European meteorological organizations.

The tables will be in royal quarto, and will cover about 400 pages. The price of the work, to be published by Gauthier-Villars, will be 35 francs.

The Council are requested by the gentlemen who have prepared the tables to ascertain the probable demand for the work in this country, and I am therefore to request through your columns that any intending purchaser will send his name to me.

ROBERT H. SCOTT.

Meteorological Office, 116 Victoria Street, London, S.W., February 16.

PLAN OF TABLES.

CHAPTER I.

Section I. Length.

1. French lines	to mm.	...	0—100 lines
2. ,,	,, English inches	...	,,
3. French inches and lines	,, mm.	...	20—30 inches
4. French lines	,, English inches	...	250—350 lines
5. English inches	,, mm.	...	0—100 inches
6. ,,	,, ,,	...	17—32 ,,
			(in ·001 inch)
7. mm.	,, English inches	...	0—100 mm.
8. ,,	,, ,,	...	440—800 ,,
9. Russian half-lines	,, mm.		
10. ,, ,,	,, English inches		
11. French feet	,, metres		
12. ,,	,, English feet		
13. English feet	,, metres		
14. Metres	,, English feet		
15. Kilometres	,, English miles		
16. English miles	,, kilometres		

Section II. Weight.

1. Grains	to grammes
2. Grammes	,, grains

Section III. Time and Angular Measure.

1. Days of year	to decimals of year and to angles
2. Hours	,, ,, ,,
3. Minutes	,, ,, ,,
4. Hours	,, decimals of day
5. Minutes	,, ,, ,,
6. Seconds	,, ,, ,,
7. Minutes or seconds	,, decimal of the hour or minute
8. Seconds	,, decimals of hour
9. Longitude	,, time
10. Time	,, longitude

CHAPTER II.—GEODETICAL.

1. Variation of gravity with latitude and altitude.
2. Degrees on the meridian.
3. ,, ,, on circles of latitude.
4. Duration of sunshine.

Weight and Mass.

THE review of Kennedy's "Mechanics of Machinery" in NATURE, December 29, 1887 (p. 195), strikes at least one responsive chord on this side of the world. There are some questions in reference to the nomenclature of dynamics which "will not down" until they are "downed" by a convention or agreement between those who have to do with the theory of mechanics and those who have to do mostly with practice, and in this some concessions will doubtless be necessary on both sides. While in hearty sympathy with much that the reviewer says in his discussion of dynamical terms (the book under notice I have not yet seen), I wish to dissent from and to protest against one of his leading propositions.

It must be admitted that in the "vernacular" the word pound is used in two distinct senses—that is, as a unit of force and a unit of mass. Authors of mathematical treatises have sometimes, and perhaps unconsciously, ignored the latter meaning, and at other times have failed to recognize the former.

The proposition of the reviewer is to eliminate the word *mass* altogether and to use weight in its stead. To accomplish this he is obliged to use the word *weight* as meaning what is now generally expressed by the word *mass*. This, it seems to me, would be a grave error. Is it not true that *weight*, as understood by both the "learned and the unlearned" always carries with it the idea of force, the force of attraction between the earth and the particular body under consideration? And is it not also true that there are many problems in the work of the practical engineer in which *mass*, in the ordinarily accepted sense, is the essential element, rather than weight, in the ordinarily accepted sense? In short, in my judgment, the engineer *does* require the word "mass," and he also needs the word "weight." It is a misfortune when one word must be used to mean two entirely different things (as is the case of the word "pound"), and we ought to congratulate ourselves that we have the words "mass" and "weight" so commonly and generally used to represent two distinct ideas. To discard one of them and force the other into its place would be to introduce confusion rather than order. To satisfy the requirements of both mathematical or theoretical and practical convenience I have been accustomed to use the following:—

The word *pound* is used in two senses; it may mean a unit of mass or a unit of force. It is always easy by the context to tell in which sense it is used.

As a unit of force it *has not yet been accurately defined*, but it means, in general, a force equal to the attraction between the earth and a mass of one pound. As this attraction varies slightly, the pound as a unit force cannot be regarded as absolutely constant, but is sufficiently so for practical purposes.

When, by a convention of authorities, the conditions under which this attraction is accepted as equal to one pound are prescribed, it will become an invariable unit.

There are in the English system two units of force, the poundal and the pound. There are also two units of work, the foot-poundal and the foot-pound; each is the work done by the corresponding unit of force working through a distance of one foot.

The ordinary equations of dynamics, when the foot-pound-second units are used, give results in poundals or foot-poundals, which may at once be reduced to pounds or foot-pounds.

The above is open to the objection that the pound as a unit of force is not constant, but the remedy for this is indicated, and the errors introduced are of no moment in "practice."

To lessen the confusion somewhat, I have often used, in writing, the symbol *lb.* to represent the unit of *mass*, and the word *pound* that of force. In my own experience the adoption of these definitions has greatly facilitated the work of students.

I entirely agree with the criticisms made upon the equation so constantly appearing, $w = mg$. To the learner it is generally "confusion confounded," and I would cheerfully join in a "boycott" against it.   T. C. MENDENHALL.

Rose Polytechnic Institute, Terre Haute, Indiana,
U.S.A., January 26.

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ONCE more Prof. Greenhill devotes a large portion of a review to emphasizing and insisting on his peculiar, and I may say extraordinary, mode of regarding the meaning of elementary terms (see NATURE, February 16, p. 361; also December 29, 1887, p. 195).

One must assume, therefore, that these views are regarded by him as useful and conducive to clearness.

I find it difficult to express strongly enough my entire dissent from such a proposition without being apparently impolite.

That engineers are entitled if they see fit to employ as their third fundamental standard of force rather than one of mass, I admit. I do not think the plan satisfactory or clear, but there are temptations towards it, and perhaps no very serious objections. My own experience of engineering students is, however, that they are beautifully uncertain whether to put g into the numerator or the denominator of a new expression, or whether to leave it out altogether; and that they generally get over the difficulty either by asking where it must go, or by seeing which plan will give an answer of most reasonable magnitude. The real rule on engineers' principles would be to put g somewhere into the expression for any quantity with which gravity has nothing to do, and to leave g out whenever gravity is primarily concerned.

But, irrespective of this standing and well-known controversy, Prof. Greenhill's attempt to simplify matters does indeed make confusion worse confounded. He says that in the vernacular the term "weight" does not mean the force with which the earth pulls a body, but does mean the body's mass or inertia.

What kind of "vernacular" can he be thinking of?

Ask any ordinary member of the British public what he or she means by the "weight" of a thing, and you will get answers such as "its heaviness," or "its heft," or the "force required to lift it," or "the difficulty of raising it," or "the pull up you must give it," or any number of such replies; but if he ever got the answer, "I mean the mass of the body, in other words its inertia, a measure of the quantity of matter the body contains," surely he would not be satisfied with this as a fair specimen of the vernacular, but would rather regard it as one of those answers so frequently given to examiners—the product of a mind so tortured by instructors that its common-sense and vernacular are completely atrophied.   OLIVER J. LODGE.

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The Composition of Water.

TWO days after the publication of my letter in NATURE (p. 390), on the composition of water, I received the Manchester

Report of the British Association, in which (p. 668) further experiments by Dr. A. Scott are reported. Dr. Scott has succeeded in reducing the amount of nitrogen present as impurity to 1 part in 15,000, and the ratio of hydrogen to oxygen which he calculates from the newer and more accurate experiments is 1·996 or 1·997 to 1·000. This ratio agrees very well with that deduced by me from the older experiments, but is considerably higher than the ratio previously adopted by Dr. Scott, and quoted by Prof. Thorpe in his article on the composition of water.   SYDNEY YOUNG.

University College, Bristol.

ON THE DIVISORS OF THE SUM OF A GEO- METRICAL SERIES WHOSE FIRST TERM IS UNITY AND COMMON RATIO ANY POSI- TIVE OR NEGATIVE INTEGER.

"Nein! Wir sind Dichter." [1]
—*Kronecker in Berlin.*

A REDUCED ·Fermatian,[2] $\frac{r^p - 1}{r - 1}$, is obviously only another name for the sum of a geometrical series whose first term is unity and common ratio an integer, r.

If p is a prime number, it is easily seen that the above reduced Fermatian will not be divisible by p, unless $r - 1$ is so, in which case (unless p is 2) it will be divisible by p, but not by p^2.

This is the theorem which I meant to express in the footnote to the second column of this journal for December 15, 1887, p. 153, but by an oversight, committed in the act of committing the idea to paper, the expression there given to it is erroneous.

Following up this simple and almost self-evident theorem, I have been led to a theory of the divisors of a reduced Fermatian, and consequently of the Fermatian itself, which very far transcends in completeness the condition in which the subject was left by Euler (see Legendre's "Theory of Numbers," 3rd edition, vol. i., chap. 2, § 5, pp. 223-27, of Maser's literal translation, Leipzig, 1886,[3] and must, I think, in many particulars be here stated for the first time. This theory was called for to overcome certain difficulties which beset my phantom-chase in the chimerical region haunted by those doubtful or supposititious entities called odd·perfect numbers. Whoever shall succeed in demonstrating their absolute non-existence will have solved a *problem of the ages* comparable in difficulty to that which previously to the ·labours of Hermite and Lindemann (whom ·I am wont to call the Vanquisher of PI, a prouder title in my eyes than if he had been the conqueror at Solferino or Sadowa) environed the subject of the quadrature of the circle. Lambert had proved that the Ludolphian [4] number could not be a

[1] Such were the pregnant words recently uttered by the youngest of the splendid tr.umvirate of Berlin, when challenged to declare if he still held the opinion advanced in· his early inaugural thesis (to the effect that mathematic consists exclusively in the setting out of self-evident truths,—in fact, amounts to no more than showing that two and two make four), and maintained unflinchingly by him in the face of the elegant raillery of the late M. Duhamel at a dinner in Paris, where his interrogat·r —the writer of these lines—was present. This doctoral thesis ought to be capable of being found in the archives of the University (I believe) of Breslau.

[2] The word Fermatian, formed in analogy with the words Hessian, Jacobian, Pfaffian, Bezoutiant, Cayleyan, is derived from the name of Fermat, to whom it owes its existence among recognized algebraical forms.

[3] I find, not without surprise, that some of the theorems here produced, including the one contained in the corrected footnote, have been previously stated by myself in a portion of a paper "On certain Ternary Cubic Form Equations," entitled "Excursus A—On the Divisors of Cyclotomic Functions" (*American Journal of Mathematics*, vol. ii., 1879, p. 357) the contents and almost the existence of which I had forgotten : but the mode of presentation of the theory is different, and I think clearer and more compact here than in the preceding paper ; the concluding theorem (which is the important one for the theory of perfect numbers) and the propositions immediately leading up to it in this, are undoubtedly not contained in the previous paper.

I need hardly add that the term *cyclotomic* function is employed to designate the core or primitive factor of a Fermatian, because the resolution into factors of such function, whose index is a given number, is virtually the same problem as to divide a circle into that number of equal parts.

[4] So the Germans wisely name π, after Ludolph van Ceulen, best known to us by his second name, as the calculator of π up to thirty-six places of decimals.

fraction nor the square root of a fraction. Lindemann within the last few years, standing on the shoulders of Hermite, has succeeded in showing that it cannot be the root of any algebraical equation with rational coefficients (see Weierstrass' abridgment of Lindemann's method, *Sitzungsberichte der A.D.W. Berlin*, Dec. 3, 1885).

It had already been shown by M. Servais ("Mathesis," Liège, October 1887), that no one-fold integer or two-fold odd integer could be a perfect number, of which the proof is extremely simple. The proof for three-fold and four-fold numbers will be seen in articles of mine in the course of publication in the *Comptes rendus*, and I have been able also to extend the proof to five-fold numbers. I have also proved that no odd number not divisible by 3 containing less than eight elements can be a perfect number, and see my way to extending the proof to the case of nine elements.

How little had previously been done in this direction is obvious from the fact that, in the paper by M. Servais referred to, the non-existence of three-fold perfect numbers is still considered as problematical ; for it contains a "Theorem" that if such form of perfect number exists it must be divisible by fifteen : the ascertained fact, as we now know, being that this hypothetical theorem is the first step in the *reductio ad absurdum* proof of the non-existence of perfect numbers of this sort (see NATURE, December 15, 1887, p. 153, written before I knew of M. Servais' paper, and recent numbers of the *Comptes rendus*).

But after this digression it is time to return to the subject of the numerical divisors of a reduced Fermatian.

We know that it can be separated algebraically into as many irreducible functions as there are divisors in the index (unity not counting as a divisor, and a number being counted as a divisor of itself), so that if the components of the index be a^a, b^β, c^γ, . . . the number of such functions augmented by unity is

$$(a + 1)(\beta + 1)(\lambda + 1). \quad . \quad . \quad . \quad .$$

All but one of these algebraical divisors, with the exception of a single one, will also be a divisor of some other reduced Fermatian with a lower index : that one, the core so to say (or, as it is more commonly called, the irreducible primitive factor), I call a cyclotomic function of the base, or, taken absolutely, a cyclotome whose index is the index of the Fermatian in which it is contained.

It is obvious that the whole infinite number of such cyclotomes form a single infinite complex. Now it is of high importance in the inquiry into the existability of perfect numbers to ascertain under what circumstances the divisors of the same reduced Fermatian, *i.e.* cyclotomes of different indices to the same base can have any, and what, numerical factor in common. For this purpose I distinguish such divisors into superior or external and inferior or internal divisors, the former being greater, and the latter less, than the index.

As regards the superior divisors, the rule is that any one such cannot be other than a unilinear function of the index (I call $kx + 1$ a unilinear function of x, and k the unilinear coefficient) and that a prime number which is a unilinear function of the index will be a divisor of the cyclotome when the base in regard to the index as modulus is congruous to a power of an integer whose exponent is equal to the unilinear coefficient.

As regards the inferior divisors, the case stands thus. If the index is a prime, or the power of a prime, such index will be itself a divisor. If the index is not a prime, or power of a prime, then the only possible internal divisor is the largest element contained in the index, and such element will not be a divisor unless it is a unilinear function of the product of the highest powers of all the other elements contained in the index.

It must be understood that such internal divisor in

either case only appears in the first power; its square cannot be a divisor of the cyclotome.

It is easy to prove the important theorem that no two cyclotomes to the same base can have any the same *external* divisor.[1]

We thus arrive at a result of great importance for the investigation into the existence or otherwise of perfect odd numbers, which (it being borne in mind that in this theorem the divisors of a number include the number itself, but *not* unity) may be expressed as follows:—

The sum of a geometrical series whose first term is unity and common ratio any positive or negative integer other than + 1 *or* 1 — *must contain at least as many distinct prime divisors as the number of its terms contains divisors of all kinds; except when the common ratio is* − 2 *or* 2, *and the number of terms is even in the first case, and* 6 *or a multiple of* 6 *in the other, in which cases the number of prime divisors may be one less than in the general case.*[2]

In the theory of odd perfect numbers, the fact that, in every geometrical series which has to be considered, the common ratio (which is an element of the supposed perfect number) is necessarily odd prevents the exceptional case from ever arising.

The establishment of these laws concerning the divisors and mutual relations of cyclotomes, so far as they are new,

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[1] The proof of this valuable theorem is extremely simple. It rests on the following principles:—

(1) That any number which is a common measure to two cyclotomes to the same base must divide the Fermatian to that base whose index is their greatest common measure. This theorem need only to be stated for the proof to become apparent.

(2) That any cyclotome is contained in the quotient of a Fermatian of the same index by another Fermatian whose index is an aliquot part of the former one. The truth of this will become apparent on considering the form of the linear factors of a cyclotome.

Suppose now that any prime number, k, is a common measure to two cyclotomes whose indices are PQ, PR respectively, where Q is prime to R, and whose common base is Θ. Then k must measure $\Theta^P - 1$ and also $\dfrac{\Theta^{PQ} - 1}{\Theta^P - 1}$; it will therefore measure Q, and similarly it will measure R; therefore $k = 1$ [unless Q = 1 or R = 1; for suppose Q = 1, then $\dfrac{\Theta^{PQ} - 1}{\Theta^P - 1}$ is unity, and no longer contains the *core* of $\Theta^{PQ} - 1$]. Hence k being contained in R can only be an internal factor to one of the cyclotomes (viz. the one whose index is the greater of the two). [See footnote at end.]

The other theorem preceding this one in the text, and already given in the "Excursus," may be proved as follows:—

Let k, any non-unilinear function of P, the index of a cyclotome χ, be a divisor thereto. Then, by Euler's law, there exists some number, μ, such that k divides $x^{\frac{P}{\mu}} - 1$, but the cyclotome is contained algebraically in $\dfrac{x^P - 1}{x^{\frac{P}{\mu}} - 1}$; hence k must be continued in μ, and therefore in P. Also, k will be a divisor of $x^{\frac{P}{k}} - 1$ and of $\dfrac{x^P - 1}{x^{\frac{P}{k}} - 1}$, which contain $x^{\frac{P}{\mu}} - 1$ and χ respectively; consequently, if k is odd, k^2 will not be a divisor of $\dfrac{x^P - 1}{x^{\frac{P}{k}} - 1}$, and *a fortiori* not of χ. [A proof may easily be given applicable to the case of $k = 2$.]

Again, let $P = Q k^i$, where Q does not contain k. Then, by Fermat's theorem, $x^{k^i} \equiv x$ [mod. k], and therefore k divides $x^Q - 1$; but it is prime to Q. Hence, by what has been shown, k must be an external divisor of this function, and consequently a unilinear function of Q. Thus, it is seen that a cyclotome can have only one internal divisor, for this divisor, as has been shown, must be an element of the index, and a unilinear function of the product of the highest powers of all the other elements which are contained in the index.

For an extension of this law to "cyclotomes of the second order and conjugate species," see the "Excursus," where I find the words *extrinsic* and *intrinsic* are used instead of *external* and *internal*.

[2] A reduced Fermatian obviously may be resolved into as many cyclotomes, less one, as its index contains divisors (unity and the number itself as usual counting among the divisors). But, barring the internal divisors, all these cyclotomes to a given base have been proved to be prime to one another, and, consequently, there must be at least as many distinct prime divisors as there are cyclotomes, except in the very special case where the base and index are such that one at least of the cyclotomes becomes equal to its internal divisor or to unity. It may easily be shown that this case only happens when the base is − 2 and the index any even number, or when the base is + 2 and the index divisible by 6; and that in either of these cases there is only a single unit lost in the inferior limit to the number of the elements in the reduced Fermatian.

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has taken its origin in the felt necessity of proving a purely negative and seemingly barren theorem, viz. the non-existence of certain classes of those probably altogether imaginary entities called odd perfect numbers: the moral is obvious, that every genuine effort to arrive at a secure basis even of a negative proposition, whether the object of the pursuit is attained or not, and however unimportant such truth, if it were established, may appear in itself, is not to be regarded as a mere gymnastic effort of the intellect, but is almost certain to bring about the discovery of solid and positive knowledge that might otherwise have remained hidden.[1]   J. J. SYLVESTER.

Torquay, February 11.

LORD RAYLEIGH ON THE RELATIVE DENSITIES OF HYDROGEN AND OXYGEN.[2]

THE appearance of Prof. Cooke's important memoir upon the atomic weights of hydrogen and oxygen,[3] induces me to communicate to the Royal Society a notice of the results that I have obtained with respect to the relative *densities* of these gases. My motive for undertaking this investigation, planned in 1882,[4] was the same as that which animated Prof. Cooke—namely, the desire to examine whether the relative atomic weights of the two bodies really deviated from the simple ratio 1 : 16, demanded by Prout's law. For this purpose a knowledge of the densities is not of itself sufficient; but it appeared to me that the other factor involved, viz. the relative atomic *volumes* of the two gases, could be measured with great accuracy by eudiometric methods, and I was aware that Mr. Scott had in view a redetermination of this number, since in great part carried out.[5] If both investigations are conducted with gases under the normal atmospheric conditions as to temperature and pressure, any small departures from the laws of Boyle and Charles will be practically without influence upon the final number representing the ratio of atomic weights.

In weighing the gas the procedure of Regnault was adopted, the working globe being compensated by a similar closed globe of the same external volume, made of the same kind of glass, and of nearly the same weight. In this way the weighings are rendered independent of the atmospheric conditions, and only small weights are required. The weight of the globe used in the experiments here to be described was about 200 grammes, and the contents were about 1800 c.c.

The balance is by Oertling, and readings with successive releasements of the beam and pans, but without removal of the globes, usually agreed to one-tenth of a milligramme. Each recorded weighing is the mean of the results of several releasements.

The balance was situated in a cellar, where temperature was very constant, but at certain times the air currents, described by Prof. Cooke, were very plainly noticeable. The beam left swinging over night would be found still in motion when the weighings were commenced on the following morning. At other times these currents were absent, and the beam would settle down to almost absolute rest. This difference of behaviour was found to depend upon the distribution of temperature at various levels in the rooms. A delicate thermopile with reflecting cones was arranged so that one cone pointed towards the ceiling

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[1] Since receiving the revise, I have noticed that it is easy to prove that the algebraical resultant of two cyclotomes to the same base is unity, except when their indices are respectively of the forms $Q(kQ + 1)^{i_1}$ and $Q(kQ + 1)^{i_2}$, where $(kQ + 1)$ is a prime number, and Q any number (unity *not* excluded), in which case the resultant is $kQ + 1$. This theorem supplies the *raison raisonnée* of the proposition proved otherwise in the first part of the long footnote.

[2] A Paper read at the Royal Society on February 9.

[3] "The Relative Values of the Atomic Weights of Hydrogen and Oxygen," by J. P. Cooke and T. W. Richards, Amer. Acad. Proc., vol. xxiii., 1887.

[4] Address to Section A, British Association Report, 1882.

[5] "On the Composition of Water by Volume," by A. Scott, Roy. Soc. Proc., June 16, 1887 (vol. xlii. p. 396).

and the other to the floor. When the galvanometer indicated that the ceiling was the warmer, the balance behaved well, and *vice versâ*. The reason is of course that air is stable when the temperature increases upwards, and unstable when heat is communicated below. During the winter months the ground was usually warmer than the rest of the room, and air currents developed themselves in the weighing closet. During the summer the air cooled by contact with the ground remained as a layer below, and the balance was undisturbed.

The principal difference to be noted between my arrangements and those of Prof. Cooke is that in my case no desiccators were used within the weighing closet. The general air of the room was prevented from getting too damp by means of a large blanket, occasionally removed and dried before a fire.[1]

In Regnault's experiments the globe was filled with gas to the atmospheric pressure (determined by an independent barometer), and the temperature was maintained at zero by a bath of ice. The use of ice is no doubt to be recommended in the case of the heavier gases; but it involves a cleaning of the globe, and therefore diminishes somewhat the comparability of the weighings, vacuous and full, on which everything depends. Hydrogen is so light that, except perhaps in the mean of a long series, the error of weighing is likely to be more serious than the uncertainty of temperature. I have therefore contented myself with inclosing the body of the globe during the process of filling in a wooden box, into which passed the bulbs of two thermometers, reading to tenths of a degree centigrade. It seems probable that the mean of the readings represents the temperature of the gas to about one-tenth of a degree, or at any rate that the differences of temperature on various occasions and with various gases will be given to at least this degree of accuracy. Indeed the results obtained with oxygen exclude a greater uncertainty.

Under these conditions the alternate full and empty weighings can be effected with the minimum of interference with the surface of the globe. The stalk and tap were only touched with a glove, and the body of the globe was scarcely touched at all. To make the symmetry as complete as possible, the counterpoising globe was provided with a similar case, and was carried backwards and forwards between the balance room and the laboratory exactly as was necessary for the working globe.

In my earliest experiments (1885) hydrogen and oxygen were prepared simultaneously in a U-shaped voltameter containing dilute sulphuric acid. Since the same quantity of acid can be used indefinitely, I hoped in this way to eliminate all extraneous impurity, and to obtain hydrogen contaminated only by small quantities of oxygen, and *vice versâ*. The final purification of the gases was to be effected by passing them through red-hot tubes, and subsequent desiccation with phosphoric anhydride. In a few trials I did not succeed in obtaining good hydrogen, a result which I was inclined to attribute to the inadequacy of a red heat to effect the combination of the small residue of oxygen.[2] Meeting this difficulty, I abandoned the method for a time, purposing to recur to it after I had obtained experience with the more usual methods of preparing the gases. In this part of the investigation my experience runs nearly parallel with that of Prof. Cooke. The difficulty of getting quit of the dissolved air when, as in the ordinary preparation of hydrogen, the acid is fed in slowly at the time of working, induced me to design an apparatus whose action can be suspended by breaking an external electrical contact. It may be regarded as a Smee cell thoroughly inclosed. Two points of difference may

be noted between this apparatus and that of Prof. Cooke. In my manner of working it was necessary that the generator should stand an internal vacuum. To guard more thoroughly against the penetration of external air, every cemented joint was completely covered with vaseline, and the vaseline again with water. Again, the zincs were in the form of solid sheets, closely surrounding the platinized plate on which the hydrogen was liberated, and standing in mercury. It was found far better to work these cells by their own electromotive force, without stimulation by an external battery. If the plates are close, and the contact wires thick, the evolution of gas may be made more rapid than is necessary, or indeed desirable.

Tubes, closed by drowned stopcocks, are provided, in order to allow the acid to be renewed without breaking joints; but one charge is sufficient for a set of experiments (three to five fillings), and during the whole of the time occupied (10 to 14 days) there is no access of atmospheric air. The removal of dissolved air (and other volatile impurity) proved, however, not to be so easy as had been expected, even when assisted by repeated exhaustions, with intermittent evolution of hydrogen; and the results often showed a progressive improvement in the hydrogen, even after a somewhat prolonged preliminary treatment. In subsequent experiments greater precautions will be taken.[1] Experience showed that good hydrogen could not thus be obtained from zinc and ordinary "pure" sulphuric acid, or phosphoric acid without the aid of purifying agents. The best results so far have been from sulphuric and hydrochloric acid, when the gas is passed in succession over liquid potash, through powdered corrosive sublimate, and then through powdered caustic potash. All the joints of the purifying tubes are connected by fusion, and a tap separates the damp from the dry side of the apparatus. The latter includes a large and long tube charged with phosphoric anhydride, a cotton-wool filter, a blow-off tube sealed with mercury until the filling is completed, besides the globe itself and the Töppler pump. A detailed description is postponed until the experiments are complete. It may be sufficient to mention that there is but one india-rubber connection—that between the globe and the rest of the apparatus, and that the leakage through this was usually measured by the Töppler before commencing a filling or an evacuation.

The object of giving a considerable capacity to the phosphoric tube was to provide against the danger of a too rapid passage of gas through the purifying tubes at the commencement of a filling. Suppose the gas to be blowing off, all the apparatus except the globe (and the Töppler) being at a pressure somewhat above the atmospheric. The tap between the damp and dry sides is then closed, and that into the globe is opened. The gas which now enters somewhat rapidly is thoroughly dry, having been in good contact with the phosphoric anhydride. In this way the pressure on the dry side is reduced to about 2 inches of mercury, but this residue is sufficient to allow the damp side of the apparatus to be exhausted to a still lower pressure before the tap between the two sides of the apparatus is reopened. When this is done, the first movement of the gas is retrograde; and there is no danger at any stage of imperfect purification. The generator is then re-started until the gas (after from two to four hours) begins to blow off again.

In closing the globe, some precaution is required to secure that the pressure therein shall really be that measured by the barometer. The mercury seal is at some distance from, and at a lower level than, the rest of the apparatus. After removal of the mercury, the flow of gas is continued for about one minute, and then the tap between the dry and damp sides is closed. From three to five minutes more were usually allowed for the com-

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[1] I can strongly recommend this method. In twenty-four hours the blanket will frequently absorb two pounds of moisture.

[2] From Prof. Cooke's experience it appears not improbable that the impurity may have been sulphurous acid. Is it certain that in his combustions no hydrogen (towards the close largely diluted with nitrogen) escapes the action of the cupric oxide?

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[1] Spectrum analysis appears to be incapable of indicating the presence of comparatively large quantities of nitrogen.

plete establishment of equilibrium before the tap of the globe was turned off. Experiments on oxygen appeared to show that two minutes was sufficient. For measuring the atmospheric pressure, *two* standard mercury barometers were employed.

The evacuations were effected by the Töppler to at least 1/20000, so that the residual gas (at any rate after one filling with hydrogen) could be neglected.

I will now give some examples of actual results. Those in the following tables relate to gas prepared from *sulphuric* acid, with subsequent purification, as already described :—

Globe (14), *empty.*

Date.	Left.	Right.	Balance reading.
1887.			
Oct. 27–Nov. 5	G_{14} + 0·394	G_{11}	22·66
Nov. 7–Nov. 8	—	—	22·89
Nov. 9–Nov. 10	—	—	23·00
Nov. 11–Nov. 12	—	—	21·72

Globe (14), *full.*

Date.	Left.	Right.	Balance reading.	Barometer.	Temperature.
1887.				in.	° C.
Nov. 5–7 ...	G_{14} + 0·2400	G_{11}	20·52	29·416	14·7
Nov. 8–9 ...	G_{14} + 0·2364	G_{11}	19·77	29·830	12·3
Nov. 10–11 ...	G_{14} + 0·2360	G_{11}	19·18	22·807	11·2
Nov. 12–14 ...	G_{14} + 0·2340	G_{11}	29·51	30·135	10·3

The second column shows that globe (14) and certain platinum weights were suspended from the left end of the beam, and the third column that (in this series) only the counterpoising globe (11) was hung from the right end. The fourth column gives the mean balance reading in divisions of the scale, each of which (at the time of the above experiments) represented 0·000187 gramme. The degree of agreement of these numbers in the first part of the table gives an idea of the errors due to the balance, and to uncertainties in the condition of the exteriors of the globes. A minute and unsystematic correction depending upon imperfect compensation of volumes (to the extent of about 2 cubic centimetres) need not here be regarded.

The weight of the hydrogen at each filling is deduced, whenever possible, by comparison of the "full" reading with the mean of the immediately preceding and following "empty" readings. The difference, interpreted in grammes, is taken provisionally as the weight of the gas. Thus, for the filling of Nov. 5—

$$H = 0·154 - 2·25 \times 0·000187 = 0·15358.$$

The weights thus obtained depend of course upon the temperature and pressure at the time of filling. Reduced to correspond with a temperature of 12°, and to a barometric height of 30 inches (but without a minute correction for varying temperature of the mercury) they stand thus—

November	5	...	...	...	0·15811
„	8	...	...	...	0·15807
„	10	...	...	...	0·15798
„	12	...	...	...	0·15792
		Mean	...		0·15802

The hydrogen obtained hitherto with similar apparatus and purifying tubes from hydrochloric acid is not quite

so light, the mean of two accordant series being 0·15812.

The weighing of oxygen is of course a much easier operation than in the case of hydrogen. The gas was prepared from chlorate of potash, and from a mixture of the chlorates of potash and soda. The discrepancies between the individual weighings were no more than might fairly be attributed to thermometric and manometric errors. The result reduced so as to correspond in all respects with the numbers for hydrogen is 2·5186.[1]

But before these numbers can be compared, with the object of obtaining the relative densities, a correction of some importance is required, which appears to have been overlooked by Prof. Cooke, as it was by Regnault. The weight of the gas is *not* to be found by merely taking the difference of the full and empty weighings, unless indeed the weighings are conducted *in vacuo*. The external volume of the globe is larger when it is full than when it is empty, and the weight of the air corresponding to this difference of volume must be *added* to the apparent weight of the gas.

By filling the globe with carefully boiled water, it is not difficult to determine experimentally the expansion per atmosphere. In the case of globe (14) it appears that under normal atmospheric conditions the quantity to be added to the apparent weights of the hydrogen and oxygen is 0·00056 gramme.

The actually observed alteration of volume (regard being had to the compressibility of water) agrees very nearly with an *a priori* estimate, founded upon the theory of thin spherical elastic shells and the known properties of glass. The proportional value of the required correction, in my case about 4/1000 of the weight of the hydrogen, will be for spherical globes proportional to a/t, where a is the radius of the globe, and t the thickness of the shell, or to V/W, if V be the contents, and W the weight of the glass. This ratio is nearly the same for Prof. Cooke's globe and for mine ; but the much greater departure of his globe from the spherical form may increase the amount of the correction which ought to be introduced.

In the estimates now to be given, which must be regarded as provisional, the apparent weight of the hydrogen is taken at 0·15804, so that the real weight is 0·15860. The weight of the same volume of oxygen under the same conditions is 2·5186 + 0·0006 = 2·5192. The ratio of these numbers is 15·884.

The ratio of densities found by Regnault was 15·964, but the greater part of the difference may well be accounted for by the omission of the correction just now considered.

In order to interpret our result as a ratio of atomic weights, we need to know accurately the ratio of atomic volumes. The number given as most probable by Mr. Scott, in May 1887,[2] was 1·994, but he informs me that more recent experiments under improved conditions give 1·9965. Combining this with the ratio of densities, we obtain as the ratio of atomic weights—

$$\frac{2 \times 15·884}{1·9965} = 15·912.$$

It is not improbable that experiments conducted on the same lines, but with still greater precautions, may raise the final number by one or even two thousandths of its value.

The ratio obtained by Prof. Cooke is 15·953 ; but the difference between this number and that above obtained may be more than accounted for, if I am right in my suggestion that his gas weighings require correction for the diminished buoyancy of the globe when the internal pressure is removed.

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[1] An examination of the weights revealed no error worth taking into account at present.
[2] *Loc. cit.*

NOTES.

THE Woolwich Examinations question, to the importance of which we again direct attention in our first article to-day, is not to be allowed to lapse. Three or four Members of Parliament who are interested in science mean to press the Government for some rational change in the rules.

IN accordance with the rule which empowers the election of nine persons annually "of distinguished eminence in science, literature, or the arts, or for public services," Dr. Lauder Brunton, F.R.S., has been elected a member of the Athenæum Club.

THE extraordinary interest and value of the botanical collections made by Signor Odoardo Beccari during a residence of several years in the Malay Archipelago, and especially in Borneo, are well known to naturalists. For some time past Signor Beccari has been occupied at Florence with the publication of his results in the work with which botanists, whether systematists or morphologists, are familiar under the name of *Malesia*. Owing to the threatened withdrawal of the modest support which the Italian Government have extended to this publication (his collections having been acquired by the State), there is some reason to fear that it may come to an abrupt termination. Under these circumstances the Bentham Trustees have placed at Signor Beccari's disposal the sum of 1000 francs, which they were informed would secure the continuance of the work for one year. In accepting this support Signor Beccari has informed the Trustees that he hesitates the less to do so as it affords the strongest possible proof of the estimation in which his labours are held in the botanical world generally.

PROF. ISAAC BAYLEY BALFOUR, of the University of Oxford, has been elected Professor of Botany at the University of Edinburgh in the room of the late Prof. Dickson. Prof. Bayley Balfour is the son of the late Prof. Balfour, Prof. Dickson's predecessor in the Chair.

M. T. RIBOT has been appointed to the new Chair of Experimental and Comparative Psychology, founded by the Paris Municipal Council at the Collège de France.

DR. F. L. PATTON succeeds Dr. McCosh as the President of Princeton College. *Science* says :—" Dr. Patton is still a young man, being but forty-five years of age, and has yet to put forth to their fullest extent his marvellous intellectual powers. We seriously question whether any College has a President of so high an intellectual stamp as Dr. Patton."

MR. GRIESBACH, the well-known geologist to the Afghan Boundary Commission, and Deputy-Superintendent of the Geological Survey of India, has been permitted to take employment under the Ameer of Afghanistan for the purpose of developing the mineral resources of the country.

MR. H. O. FORBES has just arrived in England from New Guinea. Mr. Forbes succeeded in reaching the foot of the Owen Stanley range, after the very greatest difficulties owing to the broken nature of the country. When he returned to his camp to make the necessary arrangements for ascending the range, he found it had been attacked and his people dispersed by the natives. He had the greatest difficulty in reaching the coast, and narrowly escaped with his life.

M. EDOUARD DUPONT, Director of the Brussels Natural History Museum, has just returned to Belgium, after an absence of eight months for the purpose of visiting the Congo. M. Dupont has made a very careful study of the region between the coast and the mouth of the Kassai, with a special view to its geology and natural history. The detailed results he will shortly communicate to the Brussels Societies.

THE Rev. W. H. Dallinger, F.R.S., will on Thursday next (March 8) begin a course of three lectures at the Royal Institution, on microscopical work with recent lenses on the least and simplest forms of life.

THE *Times* understands that King's and University Colleges have been informed that the Privy Council will hear them at some date after April 16 next in support of their joint petition for incorporation as the nucleus of a Teaching University for London. The Privy Council have further desired that, as the petition of the two Colleges appears to be substantially at one with that of the Teaching University Association, the Colleges should present a joint case with that Association not later than March 31.

A DEPUTATION from the School Boards of England and Wales had an interview last week with Lord Cranbrook and Sir W. Hart Dyke, to press upon their attention some considerations with regard to technical instruction. In the course of his reply to the various statements made, Lord Cranbrook said that the Technical Instruction Bill would be introduced as soon as possible. The Government, he assured the deputation, fully intended, if possible, to pass the measure, and he ventured to ask those who were interested in it, if they did not get all that they required, to be content with a beginning, and not be too anxious to press extreme conclusions which might raise opposition that did not at present exist.

M. PASTEUR, having entered the lists as a competitor for the reward of £25,000 offered by the Government of New South Wales for exterminating the superabundant rabbits, has sent three delegates with a supply of "*microbes du choléra des poules,*" with which he hopes to win the prize. Whatever may be thought of this particular remedy, there can be no doubt as to the serious nature of the plague of rabbits in Australia. During last August the rabbit inspectors travelled 20,202 miles and destroyed 2,069,128 rabbit scalps, and from January 1 to August 1 they destroyed 10,538,778 rabbit scalps. The New South Wales Parliament lately provided funds for the making of a rabbit-proof fence from Bourke to the Queensland border.

THE earthquake which caused so much alarm at Grenada on the 10th ult. was felt in many parts of the West Indies. There were oscillations at Barbados, St. Lucia, St. Vincent, Grenada, Demerara, and Trinidad, and it is said that in many places much damage was done to house property. The earthquake was also felt on the other side of the Gulf of Paria. In Guiria three houses were destroyed and the earth opened in chasms and closed again. At Yrapa the shock was so severe that a terrified old woman threw herself into the sea and was drowned.

ABOUT midnight on January 15 a shock of earthquake was felt by a party of five persons on the road four kilometres west of Trysil Church in Central Norway. The shock was accompanied by a dull rumbling noise like that of a heavy cart passing across a bridge.

ON the night of January 5 showers of ashes fell in certain parts of Elverum in Central Norway, in some places making the snow quite gray. It is surmised that the fall may have been connected with some volcanic eruption in Iceland, as has formerly been the case in this locality.

ACCORDING to the *Panama Star and Herald* a huge wave lately struck the beach at Baraçoa, Cuba. After sweeping in fully 400 feet, it flowed back to the ocean. Nearly 300 huts and houses are said to have been destroyed, but no lives were lost, for the people saw the wave coming and fled to the hills. The beach was swept clear of every habitation that stood upon it. The wave was not a tidal wave, but the result of a three days' north wind.

M. L. TEISSERENC DE BORT discusses, in the *Annales* of the French Central Meteorological Office for 1885, part iv. (Paris, 1887), the importance of the high barometric pressures of Asia for weather forecasts over Europe. The paper deals with various types of isobars existing simultaneously over Asia and Europe, illustrated by charts. The result arrived at is that European offices would derive great advantage from daily telegrams from Asia, especially from the stations already in existence in Siberia which report to St. Petersburg by wire. Hitherto the idea has generally prevailed that the movements of the atmosphere from the westward were alone useful for the prediction of weather changes over Europe.

THE Austrian Meteorological Office has just published its *Jahrbuch* for 1886. The service was established in 1847, and the first volume contained observations for 1848-49. The new series of volumes, of which the present is the twenty-third, began with the year 1864. The stations now number 380, including three abroad, and are closer together than in any other of the larger systems; there are no less than nine stations in Vienna alone. Daily observations are published for eighteen stations; for all the others monthly and yearly *résumés* are given. The Hungarian observations are published in a separate volume.

THE *Pioneer* of Allahabad mentions a circumstance connected with two recent cyclonic storms which is worthy of the attention of meteorologists. These storms struck the Scinde desert between January 24 and 30, and passed in a straight line across the continent to Cuttack at the rate of 250 to 300 miles daily. The second continued unbroken across the Bay of Bengal to Burmah. If the line thus followed were prolonged straight westward it would reach Vienna, which is about 3600 miles from Scinde. There seems (says the *Pioneer*) to have been an unusually violent atmospheric disturbance in Vienna in the early part of January, so the time and the rate of travelling would agree with the assumption that the storms were identical with that disturbance.

Two remarkable new fluorides of potassium have been discovered by M. Moissan, the isolater of fluorine. Hydrofluoric acid is well-known to be readily capable of combining with neutral fluorides to form fluorhydrates similar to that of potassium, KF . HF; indeed it was by the use of this latter compound that fluorine was eventually so successfully obtained in the free state. Moreover, the formation of such compounds has been completely accounted for by the vapour-density determinations of Kletzinsky and Mallet, who have shown that the composition of the molecule of hydrofluoric acid just above its boiling-point is H_2F_2. But M. Moissan now shows that this double fluoride of potassium is by no means the only one, that two others, KF . 2HF and KF . 3HF may be readily obtained in well-formed crystals. When dry powdered KF . HF is placed in anhydrous hydrofluoric acid, it disappears almost instantaneously, the liquid becoming sensibly warm; in fact, M. Moissan in a few moments dissolved five to six grammes in ten grammes of the acid. On cooling this mixture to $-23°$ C. white crystals separated out; these were rapidly dried between filter paper, transferred to a platinum tube closed by a paraffined cork, weighed and analyzed. The results of the analyses indicated the composition KF . 3HF. The compound was then synthetically prepared by mixing potassium fluoride and the acid in these proportions, evading any sudden rise of temperature; the liquid was subsequently warmed to 85° in a platinum capsule, but not a trace of hydrofluoric acid vapour escaped, although that substance boils at 19°. Hence it was evident that the HF was locked up in chemical combination, and this was soon observed to be the case, for on removing the source of heat, crystals began to form even while the thermometer indicated 68°; on resuming the ordinary temperature of the room, the whole became a mass of interlaced crystals, which analysis

proved to be those of KF . 3HF. These crystals are extremely deliquescent, being decomposed by water into the free acid and potassium fluoride, emitting the acid fumes in a humid atmosphere, and dissolving in water with production of the most intense cold. If they are suddenly heated with crystalline silicon, the mass becomes incandescent, and a violent disengagement of silicon tetrafluoride gas occurs. The stability of this fluorhydrate was strikingly shown by placing a few crystals *in vacuo*, when even after two hours the manometer only showed a difference of 0·01 m. In a somewhat similar manner the compound KF . 2HF was isolated and found to be a liquid at 105°, but crystallizing in the cold. It is to the formation of these fluorhydrates that M. Moissan attributes the preservation of his fluorine-isolating apparatus, and the regular evolution of gaseous fluorine during the electrolysis.

DURING the last two or three years an extensive search for natural gas has been made in the United States. In a paper on the subject, just issued by the U.S. Geological Survey, Mr. J. D. Weeks says the results of this exploration indicate:—(1) That along the Atlantic coast, east of the Appalachian Chain, including in this term the Green Mountains, no gas is found, or, if found at all, in such small quantities as to indicate that it is of comparatively recent origin. It is also found in such horizons, and under such conditions, as to give but little evidence that it is in such storage reservoirs as to promise any considerable supply. (2) That the chief sources of the supply of natural gas in the United States are to be found in the Mississippi Valley, and, so far as present explorations show, in that portion of it east of the Mississippi River. The chief localities that had assumed any prominence as gas-centres at the close of 1886 were in South-Western New York, Western Pennsylvania, North-Western Ohio, and Central Eastern Indiana. To these may be added a locality in Michigan and one in Eastern Kansas.

IN the report of the U.S. Commission of Education for the year 1885-86, just issued, it is stated that seldom in the history of the United States have superior institutions of learning occupied so large a share of public attention or given signs of such vigorous and fruitful life as at the present time. Among these institutions are classed schools of science, pure and applied, which, according to the writer of the report, "have greatly increased the provision for superior instruction, extended its province, and borne an important part in the adjustment of its processes to the demands arising from the extraordinary increase of scientific knowledge and its applications to the leading industries of modern times."

WE have received the annual address to the Asiatic Society, Calcutta, delivered by the President, Mr. E. T. Atkinson. Speaking of the Survey of India, Mr. Atkinson says that most of the operations connected with it during the past year have been devoted to remunerative as distinguished from purely scientific investigation. In many districts the survey has been cadastral with a record of rights. The Baluchistan parties have done a considerable amount of large-scale work around Quetta and towards the Khwajah Amran range, and are now engaged on the half-inch survey of that province. The Himalayan party has been working under Colonel Tanner towards Kulu, and the Andaman party has completed the survey of the coasts of the Nicobars.

WE have received the "Geological Record for 1879," containing an account of works on geology, mineralogy, and palæontology, published during the year, with supplements for 1874-78. The volume is edited by Mr. Whitaker and Mr. W. H. Dalton, and published by Messrs. Taylor and Francis. In the preface Mr. Whitaker explains that as the position of editor of the "Geological Record" has proved to be one that can be held only with great

difficulty by a busy man who does not live in London, it has been taken over by Mr. Topley. The "Record" is to be brought up to date by giving the titles only of papers, &c., for the years 1880 to 1887. The portion for 1880 to 1884 is finished, and in great part printed ; and so large is the amount of geological literature that in this contracted form (without abstracts) two volumes will be needed for the five years.

RECENT Shanghai papers contain the report of the "Chinese Scientific Book Depot," an institution which was established three years ago for the purpose of facilitating the spread of all useful literature in the native language throughout China, and especially of books, maps, and other publications of a scientific or technical character. It does not publish works, but has merely organized a system by which the translations and compilations on scientific subjects issued by the various Departments of the Chinese central and local Governments, by missionary and other philanthropic Societies, are more widely distributed amongst the Chinese people. The demand for such books is fast increasing, and the establishment of the central depot, with branches at the more important cities, suggested itself three years since. Self-support has been the motto of the institution, and, in order to overcome Chinese prejudices, everything smacking of foreign influence has been eliminated as far as possible. During the second year a branch was opened at Tientsin, and subsequently Hangchow, Swatow, Pekin, Hankow, Foochow, and Amoy were similarly provided. During the three years about £2500 worth of books, maps, &c., have been sold, some of them finding their way to the most distant parts of China, Corea, and Japan. Taking the average price per volume at 4*d*. to 5*d*., this would give a circulation of about 150,000 volumes of useful literature, chiefly of a scientific and educational character. The shops have also served to some extent as reading-rooms, where inquirers after Western knowledge have been able to sit down and examine any works in which they felt interested. The number of scientific and other treatises already translated or compiled and published in Chinese under foreign management amounts at present to over 200. To these have been added about 250 of the most useful native works, including scientific treatises by the early Jesuit fathers.

SIR EDWARD BIRKBECK, President of the National Sea Fisheries Protection Association, is now promoting in Parliament a Bill, the object of which is to secure reasonably cheap and rapid transport for common kinds of sea fish, in quantities of 1 cwt. and upwards, from the coast to the various inland centres of population, and thus, by securing a plentiful distribution, to render an inestimable benefit alike to the poor of our inland towns and villages and the fishermen of our coast. The Bill does not attempt to interfere with the rates now charged by railway companies for prime fish, nor with quantities of less than 1 cwt. of common fish. Sir Edward Birkbeck should have no great difficulty in securing sufficient support for so moderate and good a measure.

DR. F. NANSEN, of the Bergen Museum, Norway, who thinks of journeying across Greenland next summer from east to west, intends to land on the east coast at Cape Dan (66° N.), and proceed in a north-westerly direction to Disco Bay. He will be accompanied by three men—a Norwegian soldier well known for his prowess on *Ski*, or snow-runners, and two Lapps, probably the same who accompanied Nordenskiöld. In order to qualify himself for the contemplated task, Dr. Nansen is preparing to travel on *Ski* from Bergen to Christiania, right across the mountains of Central Norway, a feat never before accomplished by anyone.

THE additions to the Zoological Society's Gardens during the past week include an African Civet Cat (*Viverra civetta*) from South Africa, presented by Capt. Webster, R.M.S. *Hawarden Castle ;* three Barred Doves (*Geopelia striata*) from Batavia, Java, presented by Mrs. G. A. Thomson ; a Cape Crowned Crane (*Balearica chrysopelargus*) from South Africa ; a Gold Pheasant (*Thaumalea picta*) from China, deposited ; a Common Wolf (*Canis lupus* ♀) European, received in exchange ; two Red Kangaroos (*Macropus rufus*), two Suricates (*Suricata tetradactyla*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

SOLAR ACTIVITY IN 1887.—The decline in the three orders of solar phenomena, spots, faculæ, and prominences which had been so marked during 1886, and particularly during the latter part of that year, continued in 1887, and although there was no spotless period so long continued as that of November 1886 (see NATURE, vol. xxxv. p. 445), the mean spotted area for the year just passed has been much below that for the year preceding it, and faculæ and prominences have shown a similar falling off. During the first four months of 1887, sunspots were both few and small, and there were several intervals of a week or longer in which no spots were seen at all ; January 9–18, February 7–16, March 3–9, April 4–11, being such intervals. There was also very little on the sun from March 10–15, and from March 27 to April 18. But after this a revival set in and a fine group of spots was seen on the sun, May 14–23, appearing again in the three following rotations, June 5–18, July 3–14, and July 30–August 9. The days of greatest spotted area during the year were July 6, 7, and 8, but after this the spots began to decrease again, and were few and small in September, October, and November. August 23 to September 12 was a very quiet period, spots only being seen on about four days ; and October 6–17, October 28 to November 4, and November 21 to December 1, were spotless intervals. The last month of the year, however, showed a second rally, a fine group of spots being observed during its first fortnight, and another appearing as the first passed off at the west limb. On the whole the mean daily spotted area for 1887 was about two-fifths of that which it was for 1886. Comparing the results for 1885, 1886, and 1887, with the years preceding the last minimum, 1885 shows a somewhat greater mean daily spotted area than 1874, 1886 than 1875, and 1887 than 1876. If, therefore, the decline continues to proceed as during the last cycle, the next minimum will fall early in 1890.

The following figures, taken from Prof. Tacchini's tables, as given in the *Comptes rendus*, may be compared with those given for 1885 and 1886 (NATURE, vol. xxxiii. p. 398, and xxxv. p. 445) :—

	Sunspots.			Faculæ.
	Relative Frequence.	Relative Size.	Mean Daily Number of Groups.	Relative Size.
January	2·87	9·35	1·17	11·52
February	3·35	7·83	1·32	10·09
March ...	1·00	3·35	0·42	16·00
April ...	1·12	7·76	0·68	6·80
May ...	4·18	22·04	1·11	9·29
June ...	4·15	29·74	1·37	20·37
July ...	5·07	25·25	1·68	14·11
August ...	4·60	23·53	1·32	14·29
September	2·47	15·75	0·56	9·23
October	1·27	20·21	0·70	10·53
November	1·70	6·41	0·71	17·30
December	6·68	40·10	1·21	16·84

In general accord with the above figures are Wolf's "relative numbers." These are given below for 1886 and 1887, together with the monthly means of the variations in magnetic declination as observed at Milan. The agreement in the general form of the curves for spot numbers and magnetic variation has not been so close in 1887 as in some previous years, nor is the calculated mean value for the magnetic variation so near the observed as in 1885 and 1886 ; the values calculated by M. Wolf's formula being 6´·79 for 1886, and 6´·21 for 1887, but the observed being 6´·72 and 6´·61.

	Wolf's Relative Numbers (Zürich).		Variation in Magnetic Declination (Milan).	
	1886	1887	1886	1887
January	28·4 ...	13·1 ..	4·07 ...	3·71
February	23·6 ...	15·7 ...	4·91 ...	3·69
March	61·8 ...	2·7 ...	8·61 ...	6·99
April	45·9 ...	7·5 ...	9·89 ...	9·33
May	29·0 ...	17·2 ...	9·06 ...	9·30
June	25·7 ...	16·3 ...	8·37 ...	9·55
July	32·9 ...	26·2 ...	9·58 ...	10·25
August	19·0 ...	21·1 ...	8·17 ..	9·07
September... ...	17·1 ...	6·9 ...	7·61 ...	6·08
October	9·5 ...	5·4 ...	6·33 ...	6·03
November... ...	0·0 ...	4·5 ...	2·48 ...	3·07
December	15·1 ...	20·5 ...	1·61 ...	2·23
Mean	25·7 ...	13·1 ...	6·72 ...	6·61

The fluctuations in the numbers and dimensions of the prominences have not been so great as for the spots, but the prominences likewise showed a maximum in July and a decline afterwards. The highest prominence observed by Prof. Tacchini during the year was on July 2, 2½' in height. Both faculæ and prominences failed to show a depression similar to that so conspicuous in November in the numbers of the spots, or the revival these displayed in December, the faculæ thus according in their behaviour rather with the prominences than with the spots. The following figures, given by the Rev. S. J. Perry in the *Observatory* for February 1888, show the general decline in prominence activity during 1887, as compared with 1886 :—

	Mean Height of Chromosphere.	Mean Height of Prominences.	Mean Extent of Prominence Arc.
1886 ...	8·05 ...	24·78 ...	13·26
1887 ...	8·13 ...	23·86 ...	9·29

A NEW COMET.—A comet was discovered by Sawerthal on February 18. It was observed at Cape Town, February 18, 14h. 32·5m., in R.A. 19h. 11m. 32·5s., and N.P.D. 146° 3' 44''. Daily motion, R.A. + 7m. : N.P.D. – 1° 15'. Its physical appearance was as follows :—It was about the seventh magnitude, had a well-defined nucleus, and a tail a degree in length. It was visible to the naked eye.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 MARCH 4–10.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 4

Sun rises, 6h. 40m. ; souths, 12h. 11m. 45·0s. ; sets, 17h. 44m. : right asc. on meridian, 23h. 2·5m. ; decl. 6° 9' S. Sidereal Time at Sunset, 4h. 36m.

Moon (Last Quarter on March 5, 3h.) rises, 0h. 26m. ; souths, 5h. 14m. ; sets, 9h. 54m. : right asc. on meridian, 16h. 3·8m. ; decl. 15° 53' S.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° '
Mercury..	6 14 ...	12 1 ...	17 48 ...	22 51·6 ...	3 21 S.
Venus.....	5 35 ...	10 5 ...	14 35 ...	20 55·6 ...	17 37 S.
Mars	21 49* ...	3 7 ...	8 25 ...	13 56·3 ...	9 2 S.
Jupiter....	1 14 ...	5 27 ...	9 40 ...	16 16·5 ...	20 22 S.
Saturn	13 20 ...	21 18 ...	5 16* ...	8 10·1 ...	20 40 N.
Uranus ...	20 39* ...	2 13 ...	7 47 ...	13 1·8 ...	5 51 S.
Neptune..	9 11 ...	16 51 ...	0 31* ...	3 42·4 ...	17 59 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

March.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.	
			h. m.	h. m.	°	°
4 ...	49 Libræ ...	5½ ...	0 0 ...	0 30 ...	334	274
6 ...	B.A.C. 6098 ...	6 ...	2 28 ...	3 25 ...	72	200

March.	h.		
4 ...	11 ...	Jupiter in conjunction with and 3° 47' south of the Moon.
4 ...	14 ...	Mars stationary.
9 ...	22 ...	Venus in conjunction with and 0° 17' north of the Moon.

Saturn, March 4.—Outer major axis of outer ring = 44''·8 ; outer minor axis of outer ring = 16''·0 ; southern surface visible.

Variable Stars.

Star.	R.A.	Decl.			
	h. m.	° '			h. m.
T Arietis	2 42·1 ...	17 3 N. ...	Mar. 8,		M
Algol	3 0·9 ...	40 31 N. ...	,,	4, 0 1	m
			,,	6, 20 50	m
R Persei	3 22·9 ...	35 17 N. ...	,,	5,	M
λ Tauri... ...	3 54·5 ...	12 10 N. ...	,,	7, 0 20	m
			,,	10, 23 12	m
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	,,	4, 22 0	M
			,,	10, 2 0	m
R Canis Majoris...	7 14·5 ...	16 12 S. ...	,,	9, 21 25	m
S Cancri	8 37·5 ...	19 26 N. ...	,,	6, 20 59	m
δ Libræ	14 55·0 ...	8 4 S. ...	,,	7, 1 6	m
U Coronæ	15 13·6 ...	32 3 N. ...	,,	10, 4 7	m
U Ophiuchi	17 10·9 ...	1 20 N. ...	,,	5, 1 28	m
X Sagittarii ...	17 40·5 ...	27 47 S. ...	,,	4, 3 0	M
β Lyræ... ...	18 46·0 ...	33 14 N. ...	,,	7, 22 0	M
U Aquilæ	19 23·3 ...	7 16 S. ...	,,	10, 5 0	m
η Aquilæ	19 46·8 ...	0 43 N. ...	,,	9, 5 0	m
Y Cygni	20 47·6 ...	34 14 N. ...	,,	4, 19 11	m
			,,	7, 19 5	m
W Cygni	21 31·8 ...	44 53 N. ...	,,	5,	m
δ Cephei	22 25·0 ...	57 51 N. ...	,,	10, 22 0	m

M signifies maximum ; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
From Coma Berenices...	190 ...	26 N. ...	March 8.
Near η Libræ	234 ...	17 S. ...	Swift. March 7.
,, γ Herculis	244 ...	16 N. ...	Very swift. Mar. 7.

THE RELATIONS BETWEEN GEOLOGY AND THE BIOLOGICAL SCIENCES.[1]

II.

IN the remarks which I have hitherto made, I have confined myself to the purely biological aspects of palæontology. As astronomy exhibits to us the orderly working of physical and chemical laws in other and far distant orbs, so palæontology presents us with the biological phenomena of many and widely-separated periods.

But besides the biological, there are two other aspects in which fossils may be viewed ; and in these aspects their relations are almost entirely with zoological science. It is the recognition of this fact which prevents the geologist from acquiescing with the claims of biologists to treat palæontology as nothing more than a branch of their own science.

The assemblage of fossils found in a particular deposit furnishes us with the most valuable evidence concerning the conditions—such as salinity of water, depth, temperature, pressure, &c.,—under which the deposit must have been formed. And, again, in the changes which the materials of fossils can be shown to have undergone we have very accurate data for determining the succession of processes to which the materials of the deposit must have been subjected since their original accumulation.

It is true that this evidence of fossils concerning the conditions under which deposits have been formed, is of a kind which has been sadly misread in the past. Until the study of deposits which are being formed at the present day was taken up in a systematic manner, it was almost hopeless to avoid numerous sources of error ; but at the present day the advantages accruing to geology from the results of deep-sea researches, are at least as great as those which by the same means have been conferred upon biology.

It is almost needless to call attention to the fact that there are vast masses of rock, including most of the calcareous and carbonaceous, and many of the siliceous and ferruginous types, of which the materials have been accumulated entirely by the agency of living organisms ; it is impossible to study the petrology of such deposits without an acquaintance with the nature and functions

[1] Address to the Geological Society by the President, Prof. John W. Judd, F.R.S., at the Anniversary Meeting, on February 17. Continued from p. 404.

of the organisms by which they were formed. But, even in the case of many arenaceous and argillaceous deposits, living organisms have played a very important part in their formation. Much of the materials of such rocks can be shown to have been used in building the coverings of organisms, to have filled up their dead shells, or to have been passed through their bodies, before being finally buried under other masses. Rocks destitute of all traces of the solid parts of animals often abound with worm-tracks, burrows, or casts.

The study of the processes by which similar rock-masses are being formed at the present day constitutes the only safe guide to us in interpreting the structures presented by ancient rock-masses. Geologists look forward with much interest to the publication of those volumes of the *Challenger* Reports, in which Mr. Murray and M. Renard will deal with these important questions.

We may especially call attention to two classes of errors that have had much to do with the false conclusions which have been arrived at concerning the conditions under which various deposits have been formed in past geological times.

In the first place, it has been tacitly assumed that all marine organisms which come from regions bordering the equator must necessarily have lived under tropical conditions. It would be quite as reasonable to treat the mosses and dwarf willows which border the eternal snows of Chimborazo and Kilima-Njaro as tropical plants. Just as mountains rising in equatorial lands to the limit of perpetual snow exhibit on their slopes every gradation of climate from tropical to frigid, so the depths of the oceans, as we now know, exhibit a perfectly similar transition. As we go downwards not only heat, but light also, rapidly diminishes, and many forms which, because they came from equatorial regions, we have hitherto regarded as tropical, we now know to live in icy-cold water as well as in almost utter darkness.

The large size and abundant development of Cephalopods, Crustaceans, and fish we now know, from recent deep-sea researches, to be no evidence whatever of the presence either of warmth or of light ; and Sir Joseph Hooker has abundantly shown the fallacy of similar reasoning when applied to plant-life. I feel sure that, when the full consequences of these important considerations come to be appreciated, the apparent anomalies of many of the supposed climatal conditions of past geological times will altogether disappear. For my own part, I have never felt any difficulty in accepting, as fully equal to the explanation of the facts of the case, the Lyellian doctrine of climate being determined by great changes in the relative positions of the land and water of the globe.

The other cause of misconception with respect to the conditions which must have prevailed during the deposition of geological deposits consists in the acceptance of an utterly false proposition, which, though seldom formulated, is often tacitly acted upon ; namely, " If two organisms exhibit similarity of structure, their environment must have been the same."

There never has been wanting abundant evidence of the fallacy of this doctrine. The general structure of the piscivorous bear of the Arctic regions, and of the frugivorous bear of the Malay peninsula, the osteology of the deer of Lapland and of India respectively, exhibit no such differences as would lead us to infer their diversity of habits and surroundings. It has long been known that elephants, rhinoceroses, and hippopotami, with lions, tigers, and hyænas, flourished under Arctic conditions. The deep-sea researches have so added to our knowledge concerning the conditions under which different forms of life exist—especially those belonging to marine faunas—as to demand a complete reconsideration of the conclusions usually accepted by geologists. For there is a general consensus of opinion among the naturalists who have studied the different groups of the deep-sea faunas, that, contrary to what might have been anticipated from the very remarkable conditions under which they live, the deep-sea forms belong, for the most part, to the same families, and often indeed to the same genera, as shallow-water forms.

The bearing of this important conclusion upon the great problem of the distribution of marine forms of life is obvious. Botanists have naturally availed themselves of the proved occurrences of colder climates in many areas to explain difficult facts of plant-distribution, such as the occurrence of well-known Arctic species on the tops of mountains in what are now temperate, or even tropical, districts. But zoologists, now that they know it to be possible for littoral forms to stray into abysmal portions of the ocean, and then subsequently, without profound modification, to re-emerge in other littoral areas, may find a clue to some

very remarkable facts concerning the distribution of marine forms of life, without having to resort to explanations which seem necessary in the case of the terrestrial forms of life which appear to be more dependent than the marine types on the circumstances of their environment.

The whole problem of the distribution of marine forms of life requires indeed to be worked out afresh on the basis of these new discoveries ; and when this is done, the first to profit by the new generalizations will be geologists, who have long been confronted by seemingly insuperable difficulties in connection with this problem.

As for the very prevalent notions that Ammonites and Belemnites, Trigoniæ and Brachiopods, with Ichthyosaurs, Pliosaurs, and Plesiosaurs, could only have lived in warm, if not actually tropical, climates, I know of no grounds whatever for any such belief. The nearest living allies of the invertebrates referred to flourish at considerable depths in icy-cold water ; and, seeing that large marine mammals now live amid snow and ice, I cannot understand why the great marine reptiles might not have done the same. Just as little reason is there for inferring that Sigillarids, Lepidodendrids, and Calamites could only have lived in tropical jungles, as there is for the once popular notion that they flourished in an atmosphere supplied with a very exceptional proportion of carbonic acid !

The sooner geologists recognize the fact that all our ideas concerning the distribution of the forms of marine life have been completely revolutionized by the discovery that there are cold and dark abysses, which are tenanted by numerous organisms having many affinities with those which live in shallow water, warmed by a tropical sun and flooded with light, the more likely will they be to avoid the errors into which we have fallen in the past. Not until the exact distribution of life-forms at different depths in the ocean has been much more perfectly worked out than it has been at present, will it be safe to reason with any confidence concerning the distribution of extinct types ; and, even then, we shall ever have to be on our guard against the prevalent fallacy which assumes that analogies in structure are indicative of similarities in the conditions of life.

And here it may be remarked that the imperfect methods employed on board the *Challenger* and most other surveying ships leave almost everything yet to be done in the way of determining the limits of depth, temperature, pressure, and other conditions under which the different forms of marine life can flourish. It is much to have obtained so great an insight into the characters of some of the creatures inhabiting the deepest parts of the ocean, and of the peculiar conditions which must exist in some of those places where marine life is abundant. But the work which has yet to be done requires the employment of dredges and nets which can be opened when they have reached a certain depth in the ocean, and which can be closed again before being drawn to the surface. Only by the employment of such apparatus can we hope to avoid those sources of error which vitiate all our present generalizations concerning the bathymetrical distribution of the existing forms of marine life.

When, in addition to these biological studies, we have equally careful determinations of the physical characters of deposits formed at varying depths and distances from the shore, and under diverse influences of tides and currents, we may hope, by combining the physical and biological evidence, to arrive at something like certain conclusions concerning the exact conditions under which various geological formations have been accumulated ; for at present our speculations upon the subject are often little better than haphazard guesses.

The conditions which must have prevailed during the deposition of a particular bed having been determined, the present mineral condition of the organic remains becomes a subject of very interesting study ; for here we may find a clue which will enable us to unravel the series of physical and chemical changes which must have gone on in the mass, since the first accumulation of its materials. In cases of difficulty of this kind, the condition of alteration of a shell or bone, of which the original composition is known, becomes an especially valuable piece of evidence.

I am convinced that the future progress of geological thought is closely bound up with the increase of our knowledge concerning the conditions under which the various forms of marine life flourish, and under which their remains become embedded in sedimentary deposits ; though what has been already accomplished in this direction, it must be admitted, is but small, and much of it will have to be done over again.

We hear much—far too much, as I think—at the present day of an "irrational uniformitarianism." Is not the real source of danger in an exactly opposite direction? Does not the irrationality characterize him who, without attempting to obtain a more complete knowledge of the processes going on during the original deposition and subsequent changes of rock-masses, is ready, as each new difficulty presents itself, to fall back upon some old discredited *Deus ex machinâ* in the form of deluges of water, floods of fire, boiling oceans, caustic rains, or acid-laden atmospheres!

Considering how little we as yet know of many of the conditions under which deposits are being formed at the present day, and remembering how large a part of the little we do know has been acquired only within the last few years, we might pause before declaring that the path upon which geology entered in earnest only some fifty years ago is a wrong one, and that the sooner we begin to retrace our steps the better.

Can we even now be in danger of forgetting that "Slough of Despond," wherein the geologist, laden with a grievous burden of traditional assumptions and irrational theories, so hopelessly floundered, till one Help pointed out a way of escape, and sent him on his way rejoicing, with the "Principles of Geology" in his hand?

The second aspect in which palæontological science presents itself to the geologist, is as affording a key to the chronology of the rock-masses of the globe. We still regard fossils as the "medals of creation," and certain types of life we take to be as truly characteristic of definite periods as the coins which bear the image and superscription of a Roman emperor or of a Saxon king.

But in the application of the principle that "strata are to be identified by their organic remains," we have now to admit as many limitations, and to exercise as much caution, as when judging of the conditions under which rock-masses must have been deposited, from the characters of the fossils which they contain.

With the restricted area of the south-west of England, where William Smith achieved his epoch-making discovery, the doctrine which he announced seemed to be absolutely true; each formation exhibited a peculiar and perfectly characteristic assemblage of organic remains, by means of which it could at once be recognized. The still more detailed studies of strata of the same age, by Hunton and Williamson in Yorkshire, by Marcou in the Jura, and by Quenstedt in Swabia, seemed to show that the principle had a wider application than even its author himself could have imagined, and that zones a few feet or even inches in thickness might be followed over considerable districts, everywhere marked by some particular type of Ammonite or other characteristic fossil.

But the more thorough and systematic study of corresponding formations over wide areas, which was inaugurated by Oppel, and has been carried on by many palæontologists since, has abundantly demonstrated that, striking as is the parallelism of the zones in such a formation as the Lias, when studied in England, France, and Germany, yet the species and varieties found on the same horizon at distant points are in many cases not identical, but merely representative; and, further, that as we pass away from any typical area, the sharp distinction between the several zones seems gradually to vanish.

The same facts come out very strikingly when we study any other great geological period. In the oldest fossiliferous strata, those of the Cambrian, nothing can be more striking than the similarity of the faunas in North America, Britain, Scandinavia, and Bohemia; and yet the species which occur at the several different horizons in these countries are certainly, for the most part, not identical, but only representative. No fact, it seems to me, could more clearly indicate that, even at that early period, there were life-provinces with a distribution of organisms in space quite analogous to that which exists at the present day.

To pass to slightly younger rocks. What can be more striking than the evidence of the limits of two life-provinces, afforded by the Calciferous strata of North America and the similar rocks of Scotland and Northern Europe, which contain the remarkable *Maclureas* and a peculiar assemblage of Cephalopods and other fossils; for these are seen at Girvan to come into close contiguity with the more southern type of Silurian, containing a very different fauna, so well seen in the Lake District and North Wales.

Another striking example of the same kind is afforded by the

Cretaceous, of which the Southern type, marked by the abundance of *Hippurites*, *Orbitolites*, and other remarkable forms, comes into close relations, as has been so well shown by Hébert, with the type which yields the ordinary Cretaceous fauna of Central Europe. In these and similar cases which might be mentioned we trace the existence of two approximating marine provinces, like those which at the present day are separated by the Isthmus of Panama.

Profs. Neumayr and Mojsisovics have indeed shown that there are good causes for believing that the distinction between the marine zoological provinces in Triassic and Jurassic times was at least as clearly marked as between the similar provinces of the present day; and the former naturalist has in addition pointed out that within the geographical provinces we have also very recognizable climatic zones.

In the year 1862, Prof. Huxley, speaking from this chair, uttered a much-needed warning against the growing practice among palæontologists of treating geological equivalence as meaning the same actual contemporaneity; and against the assumption, without positive proof, that ancient faunas and floras had an indefinite and even world-wide distribution. Palæontological discoveries during the last quarter of a century in Western North America, in India, in the Cape Colony, Australia, and New Zealand, have abundantly justified these cautions, and have shown how much such a term as "homotaxis" is needed, in order to guard against errors resulting from the abuse of the phrase "geological contemporaneity."

But when Prof. Huxley went on to suggest that "a Devonian fauna and flora in the British Isles may have been contemporaneous with Silurian life in North America and with a Carboniferous fauna and flora in Africa," I think that geologists, with the evidence they have now before them, must take exception to so sweeping a generalization. Finding, as we do, on both sides of the Atlantic the same succession of Cambrian, Ordovician, Silurian, Devonian, and Carboniferous strata, containing strikingly representative, if not identical faunas, it is impossible to doubt their general parallelism; however ready we may be to admit that the migration or development of new forms of life in the two areas need not have occurred synchronously, and that thus a certain amount of overlapping of the periods represented at distant points by the same system may exist.

On the other hand, I believe that the study of fossils from remote parts of the earth's surface has abundantly substantiated Prof. Huxley's alternative suggestion that "geographical provinces and zones may have been as distinctly marked in the Palæozoic epoch as at present." The ever-accumulating mass of palæontological evidence seems to me to be all pointing in this direction; and I confidently anticipate that the palæontological anomalies which in the past have caused so much doubt and difficulty, will, by the establishment of this principle, receive a full and satisfactory explanation.

As long ago as 1846, Darwin, in his "Observations on South America," showed that certain assemblages of fossils presented a blending of characters, which in Europe are only found apart in faunas which are of Jurassic and Cretaceous age respectively. Since that date, the study of the fossil faunas and floras of South Africa, India, Australia, New Zealand, and the Western Territories of North America has furnished an abundance of facts of the same kind, showing that no classification of geological periods can possibly be of world-wide application: that we must be contented to study the past history of each great area of the earth's surface independently, and to wait patiently for the evidence which shall enable us to establish a parallelism between the several records. Attempts to establish a universal system of nomenclature or classification of sedimentary rocks are indeed greatly to be deprecated, for if the zoological and botanical distribution of past geological times were at all comparable to that of the present day, any such universal system must be impossible.

The suggestion made to this Society by Prof. Huxley at a somewhat later date is equally valuable and important. Referring to the fauna of the Trias, he said:—"It does not appear to me that there is any necessary relation between the fauna of a given land and that of the seas of its shores. At present our knowledge of the terrestrial faunæ of past epochs is so slight that no practical difficulty arises from using, as we do, seareckoning for land-time. But I think it highly probable that, sooner or later, the inhabitants of the land will be found to have a history of their own."

The growth of our knowledge concerning the terrestrial floras and faunas of ancient geological periods, since these words were written in 1869, has constantly forced upon the minds of many geologists the necessity of a duplicate classification of geological periods, based on the study of marine and terrestrial organisms respectively.

Upon this important question the judicious remarks of my colleague, Dr. Blanford, must still be fresh in the minds of all geologists and biologists. He showed that not only are terrestrial provinces independent of marine ones, but that at the present, as well as in the past, the former are more circumscribed and have an amount of distinctness which does not exist in the case of the latter.

Nor is it difficult, in the present state of our biological knowledge, to give a reason for the existence of this state of things. Between completely separated land-areas, migration can only take place by such accidents as the transport of seeds or eggs, or as the consequence of the great but slow changes in the relations of sea and land. Forms adapted only for living in cold climates are isolated by tracts of low-lying tropical land, and, conversely, tropical forms are divided off from one another, by snow-covered mountain-chains, almost as distinctly as by actual oceans. The fact that well-known Arctic plants are found at the top of mountains in tropical or temperate lands, has seemed to many botanists as quite inexplicable without calling in the agency of a general refrigeration, like that which marked the Glacial period.

But with marine forms of life the case is totally different. The oceans are not only much larger than the continents, but they are all more or less completely connected with one another.

Forms which live at the surface of the ocean may wander freely in all directions, and know but few limitations except those imposed by temperature, absence of food, &c. ; forms living at moderate depths may migrate along shore-lines or submarine ridges from one area to another ; and even when abysmal tracts of ocean intervene between two littoral faunas, recent researches seem to show that the littoral forms of life may wander into such tracts, and eventually, perhaps, cross them, without undergoing extreme or profound modification. In this way, I think, we may account for the important fact so prominently brought into view by Dr. Blanford, that terrestrial life-provinces are and always must have been more restricted in area, and more sharply cut off from one another, than marine provinces.

With the clear recognition of this principle there falls to the ground one of the most frequently urged objections to the uniformitarian doctrines—that, namely, which is based on the supposed differences in geographical distribution in ancient times as compared with the present. I have always doubted whether there is any evidence to show that the marine life-provinces of Silurian or Carboniferous times were of greater extent than those of the present day.

I believe that the doctrine that strata can be identified by the organic remains which they contain is as sound as when it was first enunciated by William Smith ; but the problems of stratigraphical palæontology, as they now present themselves to us, are infinitely more complicated than they could possibly have seemed to him. In every fauna and flora which we are called upon to study, we have to resolve a function of three variables, these being environment, space, and time. Only after the most careful investigation, in the first place, of the complicated effects produced by the varied conditions which we group together under the term environment—temperature, food, absence of enemies, and the innumerable influences which, as we now know, determine the existence and affect the multiplication of living beings ; and by the thorough study, in the second place, of the laws of geographical distribution of plants and animals, can we hope to eliminate the effects due to environment and position, and arrive at the conclusion of what must be ascribed to time.

The task will be long, the work to be done arduous, and the efforts to be made prodigious and sustained ; but the result is one which is not hopeless and unattainable, or, indeed, even doubtful. But let us by all means remember that the real work is really only just commenced, and that we are very far indeed from our goal.

One of our greatest sources of danger to the progress of geological knowledge at the present day is the impatience which is so frequently shown at the rate of that progress, an impatience which leads to attempts to cut the tangled skeins of research by

hasty and ill-considered speculation. Geologists, no less than biologists, need to recollect and keep ever before their minds the important fact that the geological record, although it is one of enormous value, is exceedingly imperfect, and that this imperfection is quite as conspicuous in respect to physical as it is to palæontological data. How sadly is this important truth lost sight of by those who, on the strength of a few isolated facts and fragmentary observations, are prepared to construct maps of large portions of the earth's surface at far distant periods of its history. Such maps are to the geologist what " genealogical trees " are to the biologist—" will-o'-the wisps " leading us aside from the safe paths of scientific induction.

It is, I suspect, from the obvious failure of attempts of this kind—attempts which had better never have been made—that such frequent suggestions of revolt against the principles of uniformitarianism take their origin. For myself, instead of disappointment, I feel a constant surprise that these doctrines have enabled us to explain so much, when our knowledge of the causes still at work around us is still so imperfect ; and I am continually impressed by the fact that each new discovery concerning the present order of Nature removes old difficulties in the explanation of the past. In saying that I adhere to the doctrines of uniformitarianism, I, of course, mean the uniformitarianism which Lyell himself taught, and not the absurd travesty of that doctrine sometimes ascribed to him.

The well-grounded conviction which results from observing the triumph of a great principle, when applied in an overwhelming number of cases, and which refuses to abandon that principle at the first appearance of difficulty, is surely not out of place in a student of Nature. It was this scientific " faith " which led Scrope to believe, in spite of difficulties arising from the imperfect knowledge in his day of physics, chemistry, and mineralogy, that massive and schistose crystalline rocks have been formed from ordinary lavas and sediments, when subjected to enormous pressures and complicated earth-movements ; which induced Lyell to seek for and find the key to physical changes during past times in the operations going on everywhere around us ; and which finally conducted Darwin, by the application of the same principle, in the case of living beings, to the doctrine of organic evolution.

But, alas ! this " faith " seems often sadly wanting among us to-day. At a time when the mineralogical constitution of rocks and of the changes which they undergo is becoming daily more clearly revealed, when innumerable researches are throwing fresh light on the great physical processes taking place everywhere in the world around us, and when each department of biological science is contributing new "facts and arguments for Darwin," such scientific pusillanimity on the part of geologists seems, to say the least of it, singularly inopportune.

Doubtless there are difficulties still unresolved ; but does not every advance in our knowledge see the removal of some of them ? True the task of interpreting the fragmentary record of the rocks is one the end of which seems very far off ; but is not every step we take clearly an approximation towards that end ?

If any arguments were needed in favour of the continued and close co-operation of geologists and biologists, it would be found in the circumstance that the most important step in the progress of scientific thought which has been accomplished in modern times has been the direct result of a combination of geological and biological researches.

That remarkable biography, for which we are so greatly indebted to Mr. Francis Darwin, is not simply the record of a life, simple, blameless, and noble beyond that of ordinary men, the story of the workings of an intellect, truth-loving, patient, and powerful, above that of all his contemporaries ; it is the history of a most wonderful revolution in human thought—one which will perhaps be regarded in future times as the most striking event of the nineteenth century.

The grand secret of Darwin's success in grappling with the great problem of " the origin of species " is found in the fact that he was at the same time a geologist and a biologist. The concentration of the later years of his life upon zoological and botanical researches has led many to forget the position occupied by Darwin among geologists. Not only are his geological writings of the highest value for the wealth of accurate observations which they contain, and the important generalizations which they put forward ; but in his more purely biological works the value of his geological training and experience are constantly exemplified.

It was, indeed, a fortunate circumstance that Darwin, after being repelled by the narrow and soulless system of "geognosy" taught by Jameson at Edinburgh, came at Cambridge under the spell of Henslow, a man of most catholic taste, extensive acquirements, and widest sympathy with all branches of natural science. By intercourse with Henslow, Darwin's flagging interest in science was rekindled and kept alive. It is a proud boast for a University to have nourished the intellectual development of Darwin; and as that University has in the past remained faithful to the memory of Newton, making his mathematical teachings the characteristic and leading feature of its studies, so, we may hope, it will in the future aim at that complete union of geological and biological investigation of which Darwin's labours constitute so grand an example.

In the dedication of his "Journal of Researches," Darwin acknowledged "with grateful pleasure" that "the chief part of whatever scientific merit this journal and the other works of the author may possess, has been derived from studying the well-known and admirable 'Principles of Geology';" and well do I recollect how, in almost every conversation I had with him, he would enlarge with warmth of feeling upon his indebtedness to Lyell, not only for his lucid teaching, but for his constant and helpful sympathy. How did he use to speak in terms of reverence of his "master," and extol the magnanimity of one who, though twelve years his senior, had abandoned slowly and cautiously, as was the habit of his mind, yet in the end completely and ungrudgingly, his own conclusions and prepossessions, and had accepted the doctrines of a pupil.

Of Darwin's three geological books, the record of the observations made by him during the voyage of the *Beagle*, it is impossible to speak in terms of praise that will seem, to those acquainted with the merits of those admirable writings, as too high; and some portions of those works, especially the chapters dealing with the great problem of foliation, are, I am convinced, very far indeed from having received from geologists the amount of attention which they deserve.

After Darwin's return to England, in 1836, his attention was for some years almost exclusively devoted to geological researches; and it was to this Society and to its officers that he constantly came for help, advice, and sympathy. He writes at this time, "If I was not more inclined for geology than the other branches of natural history, I am sure Mr. Lyell's and Lonsdale's kindness ought to fix me."

Before reaching England, Darwin had written to Henslow from St. Helena, on July 9, 1836, asking that he might be proposed a Fellow of this Society, and on November 30 of that year he was elected. In the following February he became a member of our Council, and at the next anniversary, in 1838, undertook the duties of Secretary. This office, after he had held it for five years, he was compelled to resign through ill health: but even after he had been driven from London through the same cause, it was the evening meetings of this Society which from time to time tempted him from the seclusion of Down, till at last painful experience proved to him that he must forgo even this too-exciting pleasure. Even after being compelled to lay aside his hammer, when he had taken up scalpel and microscope to study the Cirripedia, he did not forget the fossil forms of the same group.

Whether it was the distribution of organic forms in space, or the order of their appearance in time, which had had most to do in turning Darwin's thoughts into those currents which finally led him to evolution, it would be idle to speculate; but it may safely be asserted that the geological aspects of natural history had at least as much to do with the conception of the origin of species as had the biological.

How warm was Darwin's interest, all through his life, in the progress of every branch of geological research may be gathered from his letters to Lyell and other geological friends. In what he had a presentiment would be, and which actually proved, his latest work, "The Formation of Vegetable Mould through the Action of Worms," he returned in his old age to a geological problem which had occupied him during the years of his most intimate connection with our Society.

No memories can possibly have such fascination for myself as those of the conversations which, during the last seven years of his life, I was privileged to hold with Mr. Darwin upon the current topics of geological interest. It was his habit when he came to town, twice a year, to ask me to meet him, in order to talk over geological questions, and thus I had opportunities for close intercourse and discussion. No researches in our science were too minute, none too remote from the ordinary subjects of his study, to engage his attention and command his sympathies. How keenly did he recall the pleasures of his labours in this Society, and the happiness of the friendships which he had formed here! How generously and with what warmth of appreciation did he ever speak of the labours of those who had succeeded him in endeavouring to carry out the objects of this Society! Of the gentleness, the sympathy, the contagious enthusiasm of the man, I dare not trust myself to speak!

At a time when there is perhaps some danger that the excessive specialization which seems to have become a necessity in both the geological and the biological sciences, may lead to narrowness of view, restriction of aims, and petty jealousies among the workers in circumscribed departments of those sciences, it may be well to remember how Darwin, while engaged in the most minute and detailed investigations upon barnacles, earthworms, or pigeons, upon orchids, primroses, or climbing plants, could ever keep his mind open to the influence of each new discovery in every branch of geological and biological science.

The great principles which lie at the foundation of modern geology and of modern biology are the same; and Darwin did but furnish a new testament to the old covenant already accepted by geologists. Now, more than ever in the history of natural science, is there reason for the warmest sympathy, the most thorough understanding, and the completest union in effort between the cultivators of the geological and the biological sciences. It is not by petulant unfaithfulness to the tried methods of those two sciences, and a readiness to abandon the principles which have led us to such real and important conquests, for the older methods that have been so often discredited and found wanting, that we can hope to advance those sciences.

Lyell once wrote to Darwin as follows: "I really find, when bringing up my preliminary essays in 'Principles' to the science of the present day, so far as I know it, that the great outline, and even most of the details, stand so uninjured, and in many cases they are so much strengthened by new discoveries, especially by yours, that we may begin to hope that the great principles there insisted on will stand the test of new discoveries."

And to this Darwin replied with characteristic enthusiasm:—
"*Begin to hope?* Why the *possibility* of a doubt has never crossed my mind for many a day. This may be very unphilosophical, but my geological salvation is staked upon it! . . . It makes me quite indignant that you should talk of *hoping.*"

Fifty years have elapsed since these words were written. How infinitely more complicated seem to us the problems involved in the explanation of the past by the study of the process going on around us at present, than they possibly could have done to the great pioneers of the Uniformitarian doctrines! But the reasons for Lyell's hope and Darwin's confidence are still valid, nay, are stronger than ever. For does not every new discovery remove some difficulty or supply fresh illustrations of these views? May every geologist to-day be endowed with a due share of Lyell's caution; but, for my own part, I see no reason why he should not also possess a full portion of Darwin's faith.

ON THE NUMBER OF DUST PARTICLES IN THE ATMOSPHERE.[1]

AT the beginning of the paper, reference is made to the great advance recently achieved by physiologists, regarding our knowledge of the solid matter floating in the atmosphere, as they have already provided us with a considerable amount of information regarding the number of live germs in the air under different conditions; while we have but little information regarding the dead organic and inorganic particles. The following investigation was undertaken in the hope of bringing the physical side of the subject abreast of the physiological; and in this paper is given an account of a method devised by the author for counting the dust particles in the air, and also some results obtained by means of it.

One difficulty presented in this investigation is the extreme minuteness of the particles to be counted; most of them are

not only invisible, but are beyond the highest powers of the microscope. It was therefore necessary to adopt some method of making them visible. The simplest plan of doing this is to put the air—the particles in which we wish to count—inside a glass receiver, and saturate it with water vapour ; then to supersaturate the air by slightly expanding it by means of an air-pump. When this is done, a fog is produced in the receiver, and we know that each fog particle has a dust particle as a nucleus ; if then we counted these fog particles we would get the number of the dust particles. By this process, however, we would not by any means have counted all the dust particles present, as the fog particles so formed do not represent nearly all the dust particles. If, after time has been given for these fog particles to settle, another supersaturation be made, the receiver will become packed with another set of fog particles, which would require to be counted ; and this process would require to be repeated a great number of times before the last particles would become visible and be counted. It is then shown that if there is only a little dust in the air, so that the particles are wide apart, then only one supersaturation is required to make all of them visible. Further, when there are few dust particles present the fog particles are large, and are easily seen falling like fine rain inside the receiver ; and it appeared that if these rain drops could be counted then the solution of the problem promised to be easy.

The following gives a general idea of the method adopted of working out this suggestion. A small glass receiver was connected on the one side with an air-pump and on the other with a cotton-wool filter. Inside the receiver was fixed a small stage, about 1 cm. square, on which the drops were to fall and to be counted. This stage was fixed at a distance of 1 cm. from the top of the receiver, it was ruled into little squares of 1 mm., and was examined through the top of the receiver by means of a magnifying glass. To illuminate this stage a gas flame was used, the light being concentrated on it by means of a globular lens full of water. The air in the receiver was pumped out, and filtered air admitted. This air was perfectly dust-free, and gave no condensation when expansion was made. Into this pure air was admitted a small and measured quantity of the air the particles in which we wished to count. After allowing a short time for the air to get saturated, one stroke of the pump was made, which supersaturated the air, and brought down a shower of fine rain ; while making the stroke with the pump, the stage was carefully observed through the magnifying glass, and the number of drops that fell on a square millimetre counted. This was repeated a number of times, and the average number of drops per square millimetre was obtained, and used for calculating the number of particles in the air. For every drop that fell on the square millimetre, 100 fell per square centimetre ; and as there is only 1 cm. of air above the stage that number will represent the number per cubic centimetre in the air of the receiver. Then, knowing the proportion in which the air tested was mixed with pure air, and knowing also the amount to which the air was expanded by the pump, we have all the figures necessary for making the calculation of the number of particles in the air under examination.

In constructing the apparatus the first thing to which attention was given was to design the arrangement of stage or platform on which the drops could be most easily seen and counted. The first stage tried was a small piece of glass mirror, ruled on the back into little squares. This seemed at first to give excellent results, the drops being easily seen on its surface ; but on attempting to count them its unsuitableness was at once evident—the confusion produced by the reflected images of the drops caused it to be abandoned at once. Then a mirror of very thick glass was tried, the glass being so thick that the reflected images were out of focus, but it did not give satisfactory results. Very thin mirrors made of microscope glass were then tried, but had to be rejected, because, though they brought the drops and their reflections together, they were unsuitable, being too rough and covered with fine specks on their surface ; only the most highly finished glass can be used for this purpose. The arrangement was then altered, and a transparent stage lit from beneath was tried. This stage was made of a small piece of carefully selected glass, and had the fine lines etched on its surfaces. It was, however, abandoned, as it did not give such promise as the mirror arrangement. All difficulties in the use of mirrors were at last got rid of by making them of silver, and now silver mirrors are the only kind used. They are very highly polished, care being taken to keep the rubbing marks in straight lines and in one direction ; they are ruled with fine lines at right angles to each other and at 1 mm.

apart. When a silver mirror is mounted in its place, properly adjusted and lighted, it appears, when seen through the lens, like a black surface on which the lines are quite distinct, and on which the small drops shine out brilliantly and are easily counted.

Some difficulty is experienced in keeping the stage at the proper temperature. If it is too hot, the drops on falling on it do not adhere, but present a beautiful illustration of the spheroidal condition, as they roll over its surface towards the lower side of the stage, and drop into the ruled lines, in which they continue rolling till quite evaporated. On the other hand, if the stage is too cold it gets dewed, and counting becomes impossible. Directions are given in the paper for mounting and keeping the counting stage in the best working condition.

In working the apparatus two methods have been employed of mixing the air to be tested with dust-free air. In one, the dusty air is introduced into a flask which communicates with the test receiver by means of a pipe provided with a stopcock. The small quantity of air that is to be mixed with the pure air in the receiver is displaced from this flask and driven into the receiver by means of a carefully measured quantity of water which is run into the flask. In this way the air to be tested can be measured with a fair degree of accuracy, and as the capacity of the receiver is easily obtained, the experimental errors need not be great.

In the other method of working, the test receiver is connected with a small gasometer, and the air to be tested is mixed with pure air in the gasometer. The gasometer used has a capacity of 20 litres, is carefully graduated and delicately hung, so that the air can be measured in it with a considerable degree of accuracy. In working this arrangement, 1 litre of the air to be tested is generally first mixed with 19 litres of filtered air. After mixing, nine-tenths of the mixture is let out, and the gasometer again filled up with pure air, and the mixture tested in the receiver. If the drops are still too close, more air is let out, and filtered air added till the desired condition is attained. There must not be too many particles present, or all of them will not fall when expansion is made. Till experience is gained, a check on the quantity is easily obtained by admitting filtered air, in place of the air from the gasometer, and seeing if any drops appear on expansion ; if none, then the correct number has not been exceeded.

After a satisfactory counting stage had been devised, and the apparatus got into working order, testing began, when at once difficulties presented themselves. The numbers counted in the successive tests of the same air agreed fairly well for a number of times, then all at once the process seemed to break down, and from time to time a great increase in the number was counted, far exceeding the errors of experiment ; then all would go right for a time, but only to be followed by failure before long. The first thing suspected for these and for other failures was always the joints of the pipes and the stopcocks, and time after time have the joints been remade with india-rubber solution and stopcocks cleaned and greased, but to find that they are almost never at fault.

It was then suspected that the failure might be due to the filtered air, with which we mixed the dusty air, not being perfectly freed from its dust. The filtering power of cotton-wool was therefore studied, when it was found that 1 inch of cotton-wool will filter perfectly if the air is passed very slowly through it, but that even 12 inches of cotton-wool will not check all the particles if the air is made to rush violently through it. Filters must therefore be tested under exactly the conditions in which they are to be used.

It was, however, found that though the air was only allowed to pass very slowly through even 12 inches of cotton-wool, condensation frequently took place if the expansion and therefore the supersaturation was great. It was thought that in this case the failure might be due to an imperfect action of the filter—that, while it checked most of the dust, yet it allowed the extremely small particles to pass, and that these extremely small particles became active centres of condensation when exposed to the high degree of supersaturation used in the tests. It therefore here became necessary to test whether the *size* of the particles has *practically* any effect on the degree of supersaturation necessary to cause the vapour to condense on them. From the investigations of Clerk Maxwell we have theoretical reasons for expecting that the size of the particles will have an influence of this kind, but at present we cannot say that it is sufficiently great to have a perceptible effect in an experiment such as that described.

To test this point the following experiment was therefore made. A little dusty air was mixed with filtered air, and put into the test receiver, and saturated with water vapour. An expansion of only 2 c.c. was made; this caused the formation of a fog. After these fog particles had settled, the air was returned to the receiver; and after a short time another 2 c.c. expansion was made, when other fog particles appeared. After this had been done a number of times, the density of the fog got less and less, and at last entirely ceased. After this an expansion of 5 c.c. was made; this produced a rainy condensation in the receiver, which appeared a number of times on successive expansions being made, getting less and less dense, and at last it also ceased entirely. After all condensation had stopped with the 5 c.c. expansion, the expansion was increased to 10 c.c., when another shower made its appearance, and after one or two expansions all condensation again ceased. After this condition was attained, an expansion of 150 c.c. was made with the pump, when scarcely one drop made its appearance.

It is concluded that in the above experiment we have distinct evidence that the *size* of the particle does affect the degree of supersaturation required to produce condensation on it. Because, though an expansion of 2 c.c. produced a supersaturation sufficient to cause more than one-half of the particles to become visible, yet it required a higher degree of supersaturation to cause condensation to take place on others. It is also concluded from the experiment that the failure of the air to keep clear, in the tests where high supersaturation was used, was not due to the presence of extremely small particles, as an expansion of 10 c.c. is practically great enough to produce a supersaturation sufficient to cause condensation on the smallest particles.

The failures in the tests not being due to the presence of extremely small particles, it is concluded that they are true cases of condensation without nuclei, similar to those referred to in a previous communication. It was thought that, if they were true cases of spontaneous condensation, they might be checked if the expansion was made slowly and free from shocks. And on the other hand any shock would tend to produce condensation in dust-free air if highly supersaturated. On trying this, it was found that no condensation took place if the stroke of the pump was made slowly and steadily, and that if made quickly, and the piston made to strike the cover of the cylinder violently, then copious showers were always produced in the dust-free air. Here, then, was the key to one of the difficulties, and accounted for the occasional increase in the number of the particles counted; many of the drops having no dust-nucleus. Failure from this cause is now entirely prevented by causing the air on its passage from the receiver to the pump to pass through a small opening, or better through a small cotton-wool filter; this checks all violent rush of air, and shocks, and keeps the filtered air perfectly free from condensation even when highly supersaturated.

Again, the failure of perfectly filtered air to keep free from condensation was frequently observed after the inside of the test receiver had been newly wetted. It looked as if the newly wetted sides had saturated the air more thoroughly, and that the condensation was due to the higher degree of supersaturation which took place when expansion was made. This class of failures was, however, traced to the manner of wetting the inside of the receiver. If it was done roughly, and the water splashed, then many nuclei were manufactured in the receiver; if it was done quietly, none, and no condensation followed. Another cause of failure was traced to a drop of water getting into the pipe by which the air entered, and the inrush of air tearing the water into fine spray, which became active centres of condensation.

As yet no great number of tests of air have been made under different conditions, natural or artificial; but in the following table will be found some of the results obtained by this method of counting.

Dust Particles in the Atmosphere.

Source of the Air.		Number per c.c.		Number per c. in.
Outside Air—Raining	...	32,000	...	521,000
,, ,, —Fair	...	130,000	...	2,119,000
Room	...	1,860,000	...	30,318,000
,, near ceiling	...	5,420,000	...	88,346,000
Bunsen Flame	...	30,000,000	...	489,000,000

In the first column of the table is entered the source of the air; in the second, the number of particles per cubic centimetre; and, for the benefit of those who think in English measures, the number per cubic inch is entered in the third column. The first number in the table, for ordinary outside air, was obtained on January 25, after a wet night. The number given for fair weather is an average got when the weather was in that condition. As yet far too few measurements have been made to enable us to trace any connection between the number of particles and the weather, but it is hoped that something practical may result from observations of this kind. The first number given for the air in a room is the number counted in the air of a room where gas was burning, and taken at a height of 4 feet from the floor; the other number was counted in air drawn from near the ceiling; and the last number was got in the air collected over a bunsen flame.

The value of numbers given in the table has not been carefully considered, and they are not given as absolutely correct; great accuracy, indeed, does not seem possible, when we consider the conditions; and, further, the number is constantly varying. For this reason it has not been thought necessary to make any corrections for temperature and pressure. Though we can get with a fair degree of accuracy the number of particles in the air in the test receiver, yet in all probability the calculated numbers given in the table are rather under than over estimates, as it is difficult to manipulate air without losing much of its dust. For instance, in one hour about one-half of the particles settle out of the air in the gasometer. Though the numbers do seem very large, yet so far as can be judged at present they are fairly correct, and at least represent the kind of numbers we have to deal with. It does seem strange that there may be as many dust particles in 1 cubic inch of the air of a room at night when the gas is burning as there are inhabitants in Great Britain, and that in 3 cubic inches of the gases from a bunsen flame there are as many particles as there are inhabitants in the world.

JOHN AITKEN.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Frank Smart Studentship of Botany, founded at Gonville and Caius College by Mr. F. G. Smart, M.A., M.B., and Mrs. Smart, by the transfer of £2400 Great Eastern Railway 4 per cent. Debentures, is to be awarded for the first time at the beginning of Easter Term. Candidates are to send in their names to the Master of the College, Dr. Ferrers, on or before March 20. The electors are the governing body of the College, acting after consultation with the Professor and the Reader of Botany for the time being in the University. The Studentship is to be open to all members of the University who have taken honours in the first part of the Natural Sciences Tripos, and of not more than five years' standing; but the elected Student must become a member of Gonville and Caius. No competitive examination is to be held for awarding the Studentship. The Student is to apply himself to original investigation in botany, and must be able to show that he is doing so at any time, on penalty of forfeiting the Studentship. The Studentship is to last two years, but may under special circumstances be prolonged for one year more. The regulations of the Studentship are only to be changed, after the death of Mr. and Mrs. Smart, by consent of the Council of the Linnean Society of London. A prize of £6 in books is to be given out of the interest of the fund to the undergraduate student of Caius College who shall distinguish himself most in botany at the annual College examination.

The collection of British birds' eggs made by the late Mr. J. P. Wilmot, of Trinity College, containing a specimen of the great auk's egg, and other specimen's figured in Hewitson's "British Oology," has been presented to the University by Lady Caroline and Mr. C. H. Russell, in memory of Mr. George Lake Russell, Lady Caroline's late husband.

Plans are submitted for the proposed new plant-houses at the Botanic Garden, to cost £2760, and of a laboratory in the garden, to cost £250.

SOCIETIES AND ACADEMIES.
LONDON.

Royal Society, February 9.—"The Small Free Vibrations and Deformation of a Thin Elastic Shell." By A. E. H. Love, B.A., Fellow of St. John's College, Cambridge.

In this paper the method employed by Kirchhoff and Clebsch for the treatment of a thin plane plate is applied to the case of a

thin shell, or plate of finite curvature. It is proved (1) that only for an inextensible spherical surface is the potential energy-function the same function of the changes of principal curvature as for a plane plate ; (2) that in general the shell cannot vibrate in such a manner that no line on the middle-surface is altered in length, because this condition makes it impossible to satisfy the boundary conditions which hold at a free edge ; (3) that surfaces of uniform curvature with no bounding edges are the only ones which admit of purely normal vibrations ; and (4) that vibrations in which the displacement is purely tangential are possible on all shells whose middle-surfaces are surfaces of revolution bounded by small circles. The possible modes of vibration of the spherical and cylindrical shell receive special discussion. The equilibrium of the shell is also considered.

Linnean Society, February 2.—Mr. Carruthers, F.R.S., President, in the chair.—The President called attention to the loss which the Society had sustained by the deaths of Prof. Asa Gray, Prof. Anton De Bary, and Mr. Irwine Boswell (formerly Syme) which had occurred since the date of the last meeting, and gave a brief review of the life and labours of each.—Mr. C. T. Druery exhibited a collection of abnormal British ferns, and made some remarks on the extraordinary number of named varieties which had been recognized, and which now required to be carefully examined and compared, with a view to some systematic arrangement of them. A discussion followed, in which the President, Mr. J. G. Baker, F.R.S., Dr. Murie, and others took part.—Dr. Amadeo exhibited and made some observations on a new species of *Tabernæmontana.*—A long and interesting paper was then read by Mr. Henry T. Blanford, F.R.S, on the ferns of Simla, based upon a collection which he had himself made there, "not much below 4500 feet, nor above 10,500 feet." His remarks were illustrated by a map, and by an exhibition of a number of the more noticeable ferns collected, many of which were extremely beautiful. Criticisms were offered by Mr. C. B. Clark, F.R.S., Mr. Gamble (Conservator of Forests, Northern Circle Madras) and Dr. William Schlich (Inspector-General of Forests to the Government of India).—A paper was then read by Mr. H. J. Veitch, on the fertilization of *Cattleya labiata,* var. *Mossiæ,* in which the author detailed an elaborate series of observations undertaken with the object of detecting, if possible, the act of fertilization of the ovules, to determine the time that elapses between pollination and that event, and to trace the development of the ovules into perfect seeds. After explaining the structure of the sexual apparatus of *Cattleya labiata* with the aid of drawings showing the separate parts, the processes following pollination were dealt with, first from the development of the rudiment into the perfect ovule, and then the ripening of the ovules into seeds, these processes being also illustrated by drawings made at particular stages. A discussion followed, in which Mr. J. G. Baker, Mr. H. N. Ridley, and others took part, and to their inquiries for further particulars Mr. Veitch replied.—The next paper, by Mr. Joseph S. Baly, contained descriptions of new species of *Galerucinæ,* and being of a technical character was taken as read.

Entomological Society, February 1.—Dr. D. Sharp, President, in the chair.—The President nominated Sir John Lubbock, Bart., M.P., F.R.S., Mr. Osbert Salvin, F.R.S., and the Right Hon. Lord Walsingham, F.R.S., Vice-Presidents for the session 1888 to 1889.—Mr. F. Pascoe exhibited a species of the Hemipterous genus *Ghilianella,* which he found at Pará with the young larva. He said it was the only occasion he ever saw the species with the larva, which was new to Mr. Bates.—Dr. Sharp exhibited some insects collected by Mr. A. Carson on Kavalla, an island in Lake Tanganyika. The Coleoptera were nearly all well-known species, exemplifying the fact that many of the commoner insects of tropical Africa have wide distribution there, some of these species being common to both Natal and Senegal. The most remarkable of the insects was a large Lepidopterous caterpillar.—Mr. Champion exhibited specimens of *Casnonia olivieri, Œdichirus unicolor, Paussus favieri, Colydium elongatum, Endophlœus spinulosus, Hetærius arachnoides, Pseudotrechus mutilatus; Singilis bicolor, Phyllomorpha laciniata,* recently collected by Mr. J. J. Walker, R.N., at Gibraltar, Tetuan, and Tangier.—Mr. R. South exhibited a remarkable variety of *Polyommatus phlœas,* caught by him in North Devon in 1881.—Mr. R. W. Lloyd exhibited a living specimen of a species of *Ocnera* from Ispahan.—Mons. A. Wailly exhibited, and read notes on, a number of cocoons of *Antheræa assamensis, A. roylei, Actias selene,* and *Attacus ricini,* lately received from Assam ; also a number of nests of cocoons of *Bombyx rhadama*—the silk of which is used by the Hovas in the manufacture of their stuffs called "Lambas"—from Madagascar.—Captain H. J. Elwes read a paper on the butterflies of Sikkim, the result of many years' collecting. He said he had been enabled to complete his observations during the enforced delay at Darjeeling of Mr. Macaulay's Mission to Thibet, of which he was a member. He stated the number of species occurring in this district to be about 530, which is greater than the number hitherto found in any other locality in the Old World. Of these the greater part only occur in the hot valleys at an elevation of 1000 to 3000 feet, and these are for the most part of a purely Malayan character, whilst those found in the middle zone are in many cases peculiar to the Himalayas ; and the few species from the alpine parts of the country at 12,000 to 16,000 feet are of a European or North Asiatic type. An important feature in this paper was the numerous observations taken on the habits, variation, seasons of appearance, and range of altitude of the various species, for which the author said he was largely indebted to Herr Otto Möller, of Darjeeling. The paper concluded with an analysis of the species and genera compared with those found in the North-West Himalayas and in the Malay Peninsula. Mr. J. H. Leech, Dr. Sharp, Captain Elwes, and others took part in the discussion which ensued.

Zoological Society, February 7.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's menagerie during the month of January.—Mr. E. G. Loder exhibited and made remarks on a very large African Elephant's tusk, which weighed 180 pounds, and was, as he believed, the largest tusk hitherto authentically recorded.—Mr. A. Thomson exhibited a living specimen of the larval form of Stick-Insect (*Empusa pauperata*) from the Insect House.—Mr. G. A. Boulenger, read the third of his series of contributions to the herpetology of the Solomon Islands. The collection now described have been obtained by Mr. C. M. Woodford during a visit to the islands of Guadalcanar and New Georgia. The author observed that though the collection contained over 200 specimens, only four species were thereby added to the herpetological list of the Solomons, showing that our knowledge of that fauna was approaching completion.—A communication was read from Mr. Arthur G. Butler, containing descriptions of some new Lepidoptera from Kilima-njaro. Some of the specimens described had been collected by the late Bishop Hannington, and others by Mr. F. J. Jackson.—Mr. Frank E. Beddard read a paper upon certain points in the visceral anatomy of the Lacertilia. The paper dealt principally with *Monitor,* in which the presence of a peritoneal fold covering the abdominal viscera and separating them from the lungs was referred to ; this membrane was compared with a corresponding structure in the Crocodilia.—Mr. D. D. Daly gave an account of the Birds'-nests Caves of Northern Borneo, of which no less than fifteen were now known to exist in different parts of the North Bornean Company's territories. Most of these were situated in limestone districts in the interior, but two of them were in snadstone formations near the sea-coast.—A communication was read from Mr. R. Bowdler Sharpe, containing the description of a new species of Tyrant-bird of the genus *Elainea,* from the Island of Fernando Noronha. This was proposed to be called *E. ridleyana,* after Mr. H. N. Ridley, who had obtained the specimens during his recent exploration of that island.—Mr. Osbert Salvin, F.R.S., read a note on *Ornithoptera victoriæ,* from Guadalcanar Island of the Solomon Group, and pointed out the characters which separated this species from a closely allied form of the Island of Maleite, proposed to be called *O. reginæ.*

Geological Society, February 8.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read :—On some remains of *Squatina cranei,* sp. nov., and the mandible of *Belonostomus cinctus,* from the Chalk of Sussex, preserved in the collection of Mr. Henry Willett, of the Brighton Museum, by Mr. A. Smith Woodward.—On the history and characters of the genus *Septastræa,* D'Orbigny (1849), and the identity of its type species with that of *Glyphastræa,* Duncan (1887), by Dr. George Jennings Hinde.—On the examination of insoluble residues obtained from the Carboniferous Limestone at Clifton, by Mr. E. Wethered.

Royal Microscopical Society, February 8.—Annual Meeting.—The Rev. Dr. Dallinger, F.R.S., President, in the chair.—The Report of the Council was read, showing a further increase in the number of Fellows, and in the revenue of the

Society.—Mr. Crisp referred to the great loss the Society had sustained by the death of Dr. Millar, who had always taken a lively interest in the affairs of the Society, and for nearly thirty years had been a member of Council.—Dr. Dallinger delivered his annual address.

PARIS.

Academy of Sciences, February 20.—M. Janssen in the chair.—Third note on the doctrine of probabilities as applied to target practice, by M. J. Bertrand. The object of this paper, which has been prepared at the request of several artillery officers specially interested in the subject, is to present in a form capable of immediate application the results already arrived at as set forth in the previous communications.—On the species of Proneomenia on the coast of Provence, by MM. A. F. Marion and A. Kowalevsky. In a previous note the authors described a new genus of Solenogaster from the Gulf of Marseilles differing from the Proneomenia by its thorny integument. Here he describes four distinct species of the genus Proneomenia which occur on the coast of Provence, and which present features by which they may be readily distinguished from *P. sluiteri* described by Hubrecht. These species, none of which exceed 15 mm. in length, are respectively named *P. vagans, P. caulini, P. desiderata,* and *P. aglaophenia.* Incidental reference is made to a fifth species (*P. gorgonophila*) discovered on the coast of Algeria.—Observations of the new planet Charlois, 272, made at the Observatory of Algiers with the 0·50 m. telescope, by MM. Rambaud and Sy. The observations for right ascension, declination, apparent position, &c., extend over the period February 10–11.—Observations of the same planet are also recorded for February 8–13 made at the Observatory of Marseilles with the Eichens equatorial, by M. Borrelly.—Permanent deformations and thermodynamics (continued), by M. Marcel Brillouin. The chief subjects here discussed are the principle of equivalence, specific and latent heats, and the differential relations between the specific heats.—On the electrostatic attraction of electrodes in water and attenuated solutions, by M. Gouy. The theory of the propagation of electricity in the permanent state suggests the presence of free electricity during the passage of the current, not only on the outer surface of the conductors, but also on the surface separating two conductors of different specific resistance, the electric force necessarily having different values on either side of this surface. The author here endeavours to ascertain whether this hypothetic layer of free electricity on the contact surface might be capable of exercising any electrostatic actions. For this purpose he studies the case of two metallic conductors placed in a moderately conducting liquid and maintained by a pile with different potentials, in order to determine how far they may be acted upon by appreciable forces. His experiments seem to show that these forces really exist, and are in fact much more considerable than could have been foreseen.—On the coefficients of proportionality in radiating heat, by M. L. Godard. The experiments here described seem to show that the coefficients of proportionality given by the study of the diffusion of heat, and confirmed by the spectro-photometric analysis of coloured substances, are the same as the numbers obtained by M. L. Mouton in his researches on the distribution of heat in the normal spectrum of the sun.—Preparation and properties of a bi-hydrofluate and of a tri-hydrofluate of fluoride of potassium, by M. H. Moissan. While hydrochloric acid yields with difficulty the hydrochlorates of chlorides, hydrofluoric acid combines readily with the neutral fluorides to produce hydrofluates of the general formula KFl, HFl. But these compounds, including 1 equivalent of hydrofluoric acid, are not the only ones that may be obtained, at least with the alkaline metals. The author has succeeded in preparing two new combinations containing 2 and 3 equivalents of acid for 1 of fluoride of potassium. These combinations, abounding in hydrofluoric acid, and capable of being kept in the fluid state at temperatures ranging from 65° to 105° C., may perhaps under certain conditions enable the hydrofluoric acid to react readily on a certain number of organic or mineral compounds.—On a new reagent of the products of saponification of cotton-oil, by M. Ernest Milliau. The chemical reagent here described, which is not observed in the fatty acids of olive-oil, is so sensitive that by its means the presence may easily be detected of 1 per cent. of cotton-oil in olive-oil. All risk of error is removed, as the separation is effected, not on the oil itself, but on the fatty acids free from all impurity. Science has thus supplied the long sought-for means of infallibly detecting any adultera-tion of olive-oil by cotton-oil in the proportion of from 5 to 20 per cent., as is usually practised in the trade.—On the essence of lavender, by MM. R. Voiry and G. Bouchardat. The results of the analysis of this essence differ in some respects from those hitherto published. The authors have determined the presence of an oxygenated compound identical with eucalyptol, and the almost complete absence of carburets of hydrogen.—The sardine fisheries on the west coast of France in 1887, by M. Georges Pouchet. Last year was characterized by an extreme abundance of sardine on the French fishing-grounds, at the very time when the most opposite reasons were being advanced to account for a supposed gradual disappearance of the species from the French waters. On this point nothing positive can be asserted in the absence of any accurate knowledge of the migrations and spawning-grounds of the sardine.—On the Quaternary station of La Quina, Charente, by M. Emile Rivière. This station of prehistoric man, which lies near the banks of the Voultron in the Canton of La Valette, has recently been carefully explored by the author, who agrees with M. Chauvet in assigning it to the Mousterian (reindeer) epoch. The animal remains include the cave-bear, jackal, wild cat, horse, *Bos primigenius, Cervus elephas,* and especially the reindeer, in great abundance. No human bones were found, but there is an abundance of chipped flints, some very fine, and evidently worked on the spot.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Navigation and Nautical Astronomy : W. R. Martin (Longmans).—The Method of Creation : H. W. Crosskey (Sunday Sch. Assn.).—Elementary Physiography : J. Thornton (Longmans).—Life in Corea : W. R. Carles (Macmillan).—Discursive Essays on the Phenomena of the Heavens and Physical History of the Earth, Part 1 (London Literary Society)—Technological Dictionary, 3 vols.. English. German, and French : Röhrig and Schiller (Trübner).—Emin Pasha in Central Africa (Philip).—Das Antlitz der Erde, vol. ii. : E. Suess (Tempsky, Wien).—Jahrbuch der k. k. Geologischen Reichsanstalt, Jahrg. 1887, xxxvii. Band, 2 Heft ; Abhandlungen der k k. Geologischen Reichsanstalt, Jahrg. 1887, xi. Band. 2 Abthg. (Wien).—Industrial Instruction : R. Seidel (Heath, Boston).—The Manual Training School : C. M. Woodward (Heath, Boston).—A Pocket-book of Electrical Rules and Tables, 5th Edition : Munro and Jamieson (Griffin). —ii. Jahresbericht (1886) der Ornithologischen Beobachtungstationen im Königreich Sachsen : Dr. A. B. Meyer and Dr. F. Helm (Dresden).

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